A Discourse
ON
MODERN PROBLEMS RELATING TO THE ANTIQUITY OF MAN.

DELIVERED AT THE MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, HELD AT DUNDEE, SEPTEMBER, 1912.

BY

ARTHUR KEITH, M.D., LL.D., F.R.C.S.,
HUNTERIAN PROFESSOR, ROYAL COLLEGE OF SURGEONS OF ENGLAND.

ON my bookshelves there is placed a series of odd volumes containing past reports of this association, which Fortune sent my way many years ago on a Whitechapel bookstall. Among them there is one volume I prize—that which contains the history of the meeting at Aberdeen in 1859. In that volume you will find an early phase of the subject of my discourse for this evening—the Antiquity of Man. Sir Charles Lyell presided over the Section of Geology; in his opening address he announced that "a work will very shortly appear by Mr. Charles Darwin, the result of twenty years' observation and experiment," and that the evidence which had accumulated in recent years "made it probable that man was old enough to have co-existed at least with the Siberian mammoth." From other statements made in his address it is clear that Lyell was then convinced that man's appearance on earth was infinitely older than the limits fixed by Biblical record. I do not suppose I have a single listener who heard that address in Aberdeen fifty-three years ago, but even those who are not yet old will concede that the new doctrine, even as preached by Sir Charles Lyell, was not likely to be acceptable to the general membership of the Geological Section in the year 1859. You will find an exact record of what happened at the meeting—not in the official report of the year, but in the letters of Mr. William Pengelly, the explorer of Kent's Cavern. Orthodoxy was represented at the meeting by the Rev. Dr. Anderson, who, in Mr. Pengelly's words, "attempted to castigate Lyell for his opening address." There was a considerable amount of orthodoxy in the room, and Dr. Anderson got a very undue share of applause. The doctrine which Lyell and his companions championed in the face of public opprobrium in 1859 is the accepted and orthodox opinion of the vast majority of thoughtful people in the year 1912.

That splendid movement of the nineteenth century which knocked the shackles of tradition from the problem of man's origin was led by men of courage, conviction, and sound judgement. It was a progressive and victorious movement they initiated, but in every movement of that kind there comes a time when those who cleared the way turn circumspect, cautious, and more crititical than constructive. Opinion tends to become fixed and conventionalized, and then a new heterodoxy raises its head. That is the phase which we, who make a special study of the facts relating to man's origin, seem to have reached now. I cannot cite a more stalwart or distinguished representative of the orthodox opinion of to-day than Professor Boyd Dawkins of Manchester. In his Huxley Lecture of 1910 he gives very clearly his opinions on the antiquity of man—ripe convictions which are founded on a lifetime of active investigation and study. In his opinion, the history of man does not extend beyond the Pleistocene period—the phase of the earth's history which immediately precedes the one in which we live. He accepts the fossil man of Java—*Pithecanthropus*—a being with a brain a little more than half the size of a modern man's, as representative of mankind at the beginning of the Pleistocene; before the end of that period men of the modern type appeared. In Professor Boyd Dawkins's opinion, then, man was evolved during the Pleistocene period, and therefore, from a geologist's point of view, is a recent addition to the earth's fauna. If we ask how long ago it is since man appeared, Professor Boyd Dawkins replies:

> It cannot be measured in years—only by the sequence of geological events, and by the changes in animal life.

Yet we are certain that years came cycling round in the Pleistocene period just as they do now, and that every cycle wrought some degree of change on the face of the earth and on the form of living things—a degree of change which may be imperceptible in the period of a man's life, and yet cumulative and apparent in the course of time. Men who have studied the transformations effected during the Pleistocene period have formed varying estimates of its duration, but we may safely adopt as a moderate figure the 400,000 years given by Professor Sollas at a meeting of this association in 1900. We may accept, then, as the orthodox opinion of to-day, that the dawn of the very earliest form of humanity lies 400,000 years behind us; in that space of time man as we know him now was evolved from a crude, almost prehuman form.

For a representative of modern heterodoxy—as far as relates to the antiquity of man—we cannot do better than visit the Royal Natural History Museum in Brussels, and follow the guidance of M. Rutot, who has devoted himself to the study of the stone implements of ancient man and of recent geological formations. One civilization succeeded another in Pleistocene as in historical times. You will admit, when you examine the handiwork of the men of the Magdalenian age—at the close of the Pleistocene—that our ancestors were then artistic and skilled workmen. As we pass backwards in time from the Magdalenian to the Solutrean, and from the Solutrean to the Mousterian, Mousterian to Acheulean, and Acheulean to the Chellean—thus passing well beyond the mid-point of the Pleistocene—we find that although the handiwork of man changes in form and in design it does not lose in skill of execution; those flints of the remote Chellean period assure us that man had then a capable brain and a skilled hand. When, however, M. Rutot proceeds to show us the implements which were fashioned by men in the earlier parts of the Pleistocene, it is very probable that our orthodox companions will pull out their watches and find they have pressing engagements elsewhere. Human workmanship becomes cruder as we approach the commencement of the Pleistocene. The stones which have been wrought by man's hand (eoliths) become then more difficult to distinguish from those which have been shaped by natural forces. M. Rutot, however, is convinced that he has traced man, by means of his eolithic culture, not only to the commencement of the Pleistocene, but into and through the two long geological periods which preceded the Pleistocene—the Pliocene and Miocene—and even well into the formations of the still older period, the Oligocene. In M. Rutot's opinion the origin of mankind must be assigned to a time as early as the Oligocene period. Professor Sollas has made a provisional estimate of 900,000 years for the Pliocene, and 1,800,000 for the Miocene. On this crude estimate, the heterodox opinion as to the antiquity of man must be placed at over 3,000,000 years. It is only just to M. Rutot to state that he would by no means agree to the estimates given by Professor Sollas. In his opinion the duration of the Pleistocene period was not more than 139,000 years.

The modern heterodox movement, which I have sought to bring before you in the person of M. Rutot, had as its pioneer the late Professor Prestwich, a geologist whose long experience and great knowledge were tempered with a sound and conservative judgement. In 1859 he found flints on the uplands of Kent, between the Thames and the Weald, which he recognized as certainly the handiwork of man. Thousands of these eoliths have been collected by Mr. Benjamin Harrison. The deposits in which these eoliths are found were assigned by Professor Prestwich to a Pliocene date. Fifty years ago Sir Charles Lyell expressed the opinion that "signs of man's existence" would be found in the Cromer beds of East Anglia which mark the commencement of the Pleistocene period in England. Eoliths have been found not only in the Cromer beds, but also in the Pliocene formations of that district—in the Norwich crags by Mr. Clarke, and under the Red Crag by my friend Mr. Reid Moir. Thus in England heterodox opinion traces man to the commencement of the Pliocene period. I need only add that eoliths, as evidence of man's existence, are rejected by many whose opinion is entitled to our respect. The usually accepted opinion, then, is that man makes his appearance in a definitely human form about the commencement of the Pleistocene period; there are also those who refer his evolution to a much earlier period of geological history.

One thing is certain, whatever period is adopted, the time must be long enough to allow mankind to be distributed and differentiated as we now see it in the world of to-day. Modern human races, white and yellow, red, black or brown, although so different on the surface, are yet so similar in their structure and constitution that we must suppose all of them to have arisen from a common stock. Let us look at the problem in a concrete form. I will take as opposite and contrasted types of modern humanity the fair-haired, white-skinned, round-headed European and the woolly-haired, black-skinned negro of Central Africa, and set them side by side and study them from a purely zoological point of view. We must admit that both are highly specialized types; neither represents the ancestral form. Now, in seeking for the ancestral form of our breeds of dogs, of horses, or of cattle, we select one of a generalized and ancient type—such as we conceive might have been modified to produce modern breeds. We must apply the same system to human races. If we search the present world for the type of man who is most likely to serve as a common ancestor for both negro and European, we find the nearest approach to the object of our search in the aboriginal Australian. He is an ancient and generalized type of humanity; he is not the direct ancestor of either negro or European, but he has apparently retained to a greater degree than any other living race the characters of that common stock from which both European and negro arose. If, then, we accept the Australian native as the nearest approach to the common ancestor of modern mankind—and it must be admitted that it is not a low form of man we are postulating as a common ancestor—can we form any conception of the length of time which would be required to produce the African and the European from this common stock? What do we know of the rate at which mankind evolves? There is the classical instance of Egypt. During his residence in that country, Professor Elliot Smith and his colleagues—Dr. Wood-Jones and Dr. Derry—had opportunities of examining the remains of Egyptians belonging to every period—from pre-dynastic times to the present day. They had thus facilities for studying the evolution of a people over a period of at least 6,000 years—probably longer. They found evidence of an infiltration of foreign blood both from the North and from the South; they noted minor alterations in the configuration of the head and in the state of the teeth and jaws, but they could not say that the men at the end of that period were in any respect a higher or more specialized type than the inhabitants of the Nile Valley at the beginning of that period. There is no need to go beyond our own country to find evidence that the evolution of man proceeds at a slow rate. We have now material enough to form a fairly accurate conception of the physical condition of the people who lived in Britain these 4,000 years past. Were the prehistoric Britons to come amongst us now, dressed in our modern garb, they would pass unnoticed as fellow citizens. The neolithic men of France, Switzerland, and Germany were not in anywise a lower race than their successors of to-day. When we pass to examine human remains belonging to more remote periods we are confirmed in our belief that the evolution of human races is a slow process. In this country there have been found at Galley Hill, at Bury St. Edmunds, and at Ipswich human remains which belong at least to the middle part of the Pleistocene period. These remains indicate a kind of man somewhat different from ourselves, but yet of the same type. In size of brain and in complete adaptation to an upright posture they cannot be described as less highly evolved than we are. Such evidence as we have, then, leads us to believe that the evolution of a new and distinct variety of mankind requires an extremely long period of time.

If we again ask, How long will it take to evolve the African on the one hand and the European on the other from a common stock?—Australoid, we suppose, in form—it is very apparent, on our present knowledge, we must make a very considerable allowance of time. My own opinion is that the whole length of the Pleistocene—a period, we shall say, of 400,000 years—is not more than sufficient. I am thus postulating in order to explain the differentiation and distribution of modern races; that mankind, at the beginning of the Pleistocene period, had reached a physical condition which has its best modern representation in the aborigines of Australia.

Is it not possible, however, that the evolution of man's body may not be a story of slow, continuous, almost imperceptible change, but one of alternate spurt and quiescence? The human body is notoriously the subject of sport, of defects and malformations. Many of you will recall the book which Professor Bateson published eighteen years ago, entitled *Material for the Study of Variation*. The work contained many facts which seemed to indicate that the animal body was subject to violent structural changes, and that a new form of being might be produced almost at a bound. We often see men in whom there is an extra vertebra in the loins, an additional rib, or a supernumerary digit, but we now recognize that these marked structural changes are merely the extreme manifestations of a normal degree of variation of which every man's body is the subject. The bodies of men and anthropoids are notoriously liable to anatomical variation, and we are justified for that reason in regarding their bodies as particularly plastic material in the hands of evolution. When, however, we come to examine the anatomical differences which separate one race of men from another, we see that racial characters comprise, not those marked variations which so frequently are seen by the students of human anatomy, but a multitude of minor structural features such as might slowly accumulate in the course of the differentiation of one race from another. When one comes to realize the extremely complex structure and finely adjusted nature of the human brain, it becomes very apparent that any addition to the most essential structure of the human body must be the result of an extremely slow process of growth. Only one line of evidence shakes our belief in the slow rate of human evolution, and that is the study of certain diseases of growth to which man is liable. We have come to realize in recent years that we are—as regards face, figure, stature, and nature—largely what our internal glands and secretions have made us. Growth itself is definitely regulated by means of substances set free by certain glands of the body. We are absolutely certain that a marked disturbance of these glands will in the course of a few years definitely transfigure the individual to which they belong. Nature seems to have at her command a means for executing rapid advances; but when we survey what we know of man's past history, and mark the changes he is subject to in the present, we see no sign of her having resorted to such a means.

There is another route by which we may approach the problem of man's antiquity. Man does not stand alone; he has distant and rather despised relations—the great anthropoid apes. Although the structural hiatus between him and them is wide, yet when we compare the two types we see that there is a multitude of resemblances so intimate and so peculiar, that we cannot explain them except by supposing that man and the great anthropoids had a common ancestor at one stage of the earth's history. The great anthropoids have also a distant and primitive living relative—the gibbon. The gibbon in turn, while foreshadowing in his body the structural peculiarities of his more august relatives, finds his cousins by descent in more lowly forms still—the monkeys of the Old World and the monkeys of the new. Of these two groups the monkeys of the New World are the nearest to the original stock which gave rise to the higher primates. It was through such a lineage that man rose to reach his present estate. If, then, we are to ascertain the approximate date, or, to put it in other words, the possible date at which man appeared, we must first search for the earliest traces of the basal form of the higher primates which lead towards the human line. The earliest traces we have discovered as yet were described by Dr. Max Schlosser only two years ago. In the very oldest Oligocene formation of the Fayoum, Egypt, the teeth and jaws of three primates were discovered. Two of these are allied to the South American apes, the other is a forerunner of the gibbons. These Fayoum fossils are of the highest importance to the solution of our problem. Their discovery assures us that at such an early date in the evolution of mammals the South American apes and the pro-gibbons were already in existence. They are highly evolved forms, and it is not unlikely that they appeared at a much earlier date. In European strata of the period following the Oligocene—

the Miocene—many teeth and jaws of a form of gibbon' which differ only in slight and trivial details from the teeth of living gibbons, have been discovered during the past fifty years. Here, then, we have the assurance that an animal which springs closely from the stock giving rise to man has come down to us with but little change through the leagues of time marked by the Miocene, Pliocene, and Pleistocene formations. By the middle of the Miocene we know the great anthropoids were in existence; it is most unlikely that the traces we have discovered mark their first appearance. With the evolution of the great anthropoids the appearance of a human ancestry as a separate stock is possible. From every point of view it is most probable that the human stock became differentiated at the same time as the great anthropoids. On the evidence afforded by our very imperfect knowledge of fossil forms of apes we are justified in assuming that a very primitive form of man may have come into existence during the Miocene period; at the very latest during the early part of the Pliocene. Thus, when we pursue the question of man's antiquity by studying the forms of primates contained in the Tertiary strata, we find reason to extend the possible date of his origin at least a geological epoch beyond what is allowed by the strictly orthodox. We are unable, however, to find evidence in support of the more extravagant claims of the ultra-heterodox represented by M. Rutot.

There is still another and a very important line of evidence bearing on the antiquity of man. We have, in the most cursory manner, followed the evolution of various ancestral forms of ape and anthropoid from the past towards the present; I propose now to follow the history of man's evolution, so far as we yet know it, from the present into the geological past. We are all evolutionists nowadays, and it is but natural that every one of us should expect man to become more anthropoid and more brutal the further we trace him into the past. What have we found? At the close of the Pleistocene period, which even orthodox and conservative geologists admit to have come to an end some 15,000 years ago, the men of Europe in stature and in size of brain were at least our equals. In tooth, limb, and bone they were more robust. When, however, we turn our eyes to France and pass backwards in the Pleistocene to the milder epoch marked by the last or fourth of the cold cycles which subdivided that period, modern man disappears; his place is taken by a human being of an altogether different kind—a human race or species to which the name of Neanderthal has been given by international consent. During the last six years, thanks to the enthusiasm, industry, and genius of French anthropologists, the remains of four individuals of this race have been unearthed. The strata in which these remains were found contain stone implements of the type known as Mousterian and of animals belonging to a cold climate. Neanderthal man appears suddenly in this later part of the Pleistocene, and as suddenly disappears, to be replaced by modern man. It is impossible to conceive that, just at the close of the Pleistocene period, Neanderthal man was suddenly converted into modern man. Think for a minute of the interpretation you would give of the Australian strata that are being laid down now. The older deposits contain the remains of aborigines; the newer, Europeans. You do not suppose that the aborigines are suddenly transformed to European. You must apply the same interpretation to the human remains found in the later Pleistocene. There was a supersession, not a transformation of races. We must infer, then, that at the end of the Pleistocene period there were two distinct races of mankind—Neanderthal and modern. That is a fact which our French colleagues seem to grasp with difficulty.

To follow the history of modern man into the past we shall return to England. It is a mystery why Neanderthal remains have not been discovered in England; they ought to be found, and a rumour is now current that they have been found. The oldest remains so far unearthed in England all belong to the modern type of man. They take us a long way further into the Pleistocene than the era of Neanderthal man. The skull fragment, known as the Bury St. Edmunds, was found in strata containing Acheulean flints and remains of the mammoth; the 90-ft. terrace of the Thames, in which the Galley Hill man was found, contains flints of the Chellean type. The Acheulean and Chellean flint civilizations are attributed by Professor Boule—a most reliable authority—to the long temperate interval which lies between the last two of the glacial cycles of the Pleistocene, or, if we accept the evidence of Professor Penck, between the second and third cycles. If Mr. Reid Moir and I are right in regarding the human remains lately found at Ipswich as resting under a bed of undisturbed chalky boulder-clay—it is right to say that our inferences are contested—then we have carried the history of modern man a step still further back in the Pleistocene period, for the chalky boulder-clay is the product of the great cold cycle which preceded the Chellean industry. So far as the evidence in England goes, it indicates the existence of a modern type of man at least as far back as the middle of the Pleistocene period.

All we know of man in Europe near the beginning of the Pleistocene is the famous lower jaw found near Heidelberg in 1907. A complete lower jaw with its full complement of teeth can tell with certainty a great deal about the individual to which it belonged. There is not a shadow of doubt that the Heidelberg man belonged to the Neanderthal type; perhaps he may best be described as pre-Neanderthaloid, for in strength and massiveness of jaw he foreshadows the Neanderthal men whose remains are found in Europe towards the end of the Pleistocene. Of the Neanderthal race in the middle phases of the Pleistocene we have, so far, discovered no trace. Although in many features Neanderthal man shows resemblances to the anthropoids, in others he is highly specialized. The teeth of an Australian native make a nearer approach to the anthropoid condition than those of Neanderthal man.

We have knowledge of another fossil man belonging to the beginning of the Pleistocene. In 1891 Dr. Eugene Dubois discovered in Java the fossil remains of a man who in stature, posture, and gait must have been very similar to us, but so unlike us in head form that his discoverer named this new form of man—*Pithecanthropus*. The size of his brain (855 c.cm.) was little more than half the size of the brain of a well-developed modern man. The Neanderthal man described by Professor Boule had a cranial capacity of 1,600 or 1,625 c.cm. It is usual to accept the fossil man of Java as representative of his time and race, but if we do we have to suppose that, in the early part of the Pleistocene, within a comparatively short space of time, the human brain developed at an astounding and almost incredible rate. I leave the matter there, simply asking my audience to keep in mind that there did exist in the Far East at the beginning of the Pleistocene, or perhaps close of the Pliocene, a very low form of primitive man.

Thus we have a knowledge—a very imperfect knowledge—of only two human individuals near the beginning of the Pleistocene period. The one was brutal in aspect, the other certainly low in intellect. It is hard, then, to believe that in strata belonging to the period preceding the Pleistocene there could be found fossil remains of a man of quite a high and modern type. Yet the details relating to the discovery of human remains by Professor Ragazzoni in early Pliocene strata of North Italy are so circumstantial and supported that one cannot put them lightly aside. In 1860 Professor Ragazzoni was searching in undisturbed Pliocene strata for fossil shells, he discovered remains of a human skull. His discovery was received with derision. Between 1860 and 1880 he found in the same strata remains of three further individuals. The only living anthropological authority, so far as I can learn, who accepts Ragazzoni's discovery as authentic is the celebrated Italian anthropologist, Professor Sergi, of Rome. If the remains found in these strata had been of a primitive type their authenticity would never have been called in question, but as they represented individuals as highly evolved as we are, the easiest solution of the problem was to suppose that by some means these remains had been interpolated in ancient strata at a later date.

Is it, then, possible that a human being, shaped and endowed as we are, may have existed so early as the Pliocene period? If we accept as authentic all the evidence brought forward by those who have traced man backwards by means of flints which have the appearance of man's work on them, then we must admit that Pliocene man is possible, for stones, apparently artificially fashioned, have been found in strata as old as the Eocene. If, on the other hand, we examine the evidence relating to that

group of animals to which man belongs—the higher primates—the facts, so far as we know them, render the existence of man in the Eocene and Oligocene periods impossible, improbable in the Miocene period, but quite possible in the Pliocene. If, finally, we take into consideration all the evidence relating to fossil forms of man, we must confess that the antiquity of the modern form of man is still an open problem. I, for one, am convinced that we have followed him almost unchanged to at least the middle of the Pleistocene, when we find him accompanied by another form of man almost as distinct from him as the gorilla is from the chimpanzee. Still further back, at the beginning of the Pleistocene, we find at least two forms of men—the pre-Neanderthaloid of Heidelberg and the small-brained man of Java—but the representatives of modern man at this early period we do not know. It does seem to me, taking all the scraps of evidence at our disposal, the slow rate of human evolution, and the great blanks in the geological record into account, that a man as high as the Australoid of to-day was then in existence, but I cannot bring myself to believe that human individuals as highly evolved as those discovered by Professor Ragazzoni were in existence at an early part of the Pliocene period.

The problem of man's antiquity is not yet solved. The picture I wish to leave in your minds is that in the distant past there was not one kind but a number of very different kinds of men in existence, all of which have become extinct except that branch which has given origin to modern man. On the imperfect knowledge at present at our disposal, it seems highly probable that man as we know him now took on his human characters near the beginning of the Pliocene period. How long ago that is must be measured, as Professor Boyd Dawkins insists, by the changes which the earth and living things have undergone, and yet it is only human to try to find a means of measuring that period in a term of years, and the estimates at hand give an antiquity of at least a million and a half of years.

THE CHIEF USE OF THE PERITONEUM:

*A SUGGESTION.**

BY

JOHN HOWELL, M.B., B.S.LOND., F.R.C.S.ENG.,

HONORARY SURGEON, CHELTENHAM GENERAL HOSPITAL.

Two years ago I had the pleasure of giving you the first rough outline of my theory of the use of the peritoneum. In addition to its well-known uses as a mechanical lubricator for the efficient movement of the intestines, as a lymph sac, as a secretory and absorptive gland, I ventured to propound what I considered to be its greatest use, "that of a perienteric trap for the reception and annihilation of micro-organisms which *constantly* and *normally*—that is, without producing symptoms—permeate the intestinal wall. It is part of the duty of certain organs intimately connected with the peritoneal cavity, of which the liver, stomach, and intestines are the chief, to excrete the toxic products formed therein, including, in times of stress, the bacteria themselves, with the minimum risk of contamination to the remainder and more important parts of the body."

Let me recall some of the more important facts in the anatomical environment and development of the peritoneum.

THE ANATOMY OF THE COELOM.

1. *The total extent of its surface* equals one and a half times the surface of the skin, consisting of living secretory cells, varying in shape and size in different parts, intimately connected with a most extensive subendothelial lymph capillary and blood capillary plexus, and irregular ramifying lymph spaces, yet varying in the intimacy of its attachment to the organs which it covers.

2. *The visceral layer of the peritoneum* forms the outer lining of the *absorptive* portion of the alimentary canal and its outgrowths. The internal epithelial lining of this part of the tube consists of a single layer of columnar cells with basement membrane lying directly on submucous retiform or lymphoid tissue in direct communication, in spite of muscularis mucosae and muscular layers, with the above subendothelial lymph spaces and plexus. The "duct" portion of the tube, mouth, pharynx, and oesophagus is lined with squamous epithelium on a much firmer and denser rete, and has no peritoneal covering.

3. *The blood and lymph circulation* is in both cases enormous. It is true that the absorption of air and of food requires a large blood supply; it is still more true that Nature makes the utmost use of her opportunities.

(*a*) The gastro-intestinal arteries come off the aorta; the veins return to the liver without exception.

(*b*) The right and left hepatic branches of the hepatic arteries are no larger than the bronchial. The relative importance of their functions would be about the same—that is, nutritive to the framework of the organ.

(*c*) The *omental* veins also return their blood to the liver, although they have no apparent share in the absorption of nutritive material. The size of the omental blood vessels is larger than the functions hitherto ascribed to the omentum would seem to warrant.

(*d*) The *lacteal* has a blind end in the subepithelial lymphatic space of the villus; if it were simply a question of the absorption of fat globules this could be more expeditiously performed by the direct opening of a lymphatic vessel into this space, which is the case in the rest of the body.† The lacteals are joined by the lymphatic sinuses around the bases of the solitary glands by vessels from the intermuscular plexuses and the subendothelial plexus, connected with scores of glands in the mesentery and pass to the receptaculum chyli where the lymph is conveyed to the left innominate vein.

The subendothelial lymphatic spaces of peritoneum and pleura are directly continuous through the diaphragm. In the frog the peritoneal coelom communicates through stomata directly with the large dorsal lymph sac (= our perirenal and retroperitoneal lymphatic space), and thence through the lymph hearts with the systemic circulation.

The lymph from the omentum passes by way of the gastric and splenic glands to the coeliac and thence into the receptaculum chyli.

4. *The movement of the lymph* in the coelom and lymph spaces is insured by:

(*a*) The diaphragm and the intercostal muscles, the rhythmic contraction of which ceases only with life itself.
(*b*) The muscles of the abdominal wall.
(*c*) The peristaltic and vermicular movements of the alimentary canal.
(*d*) The rhythmic contraction of the spleen.

5. *Liver.*—The significant histological structure:

1. Relation of portal capillaries to the liver cells.
2. Relation of liver cells to the bile capillaries.
3. The segregation of each lobule by connective tissue in direct continuation with the subendothelial connective tissue covering the organ (Glisson's capsule).

The peritoneum covers, and is intimately connected with, almost the whole of the stomach and small intestine, liver, and spleen, and half of the pancreas (the whole in the embryo). In the stoutest person no fat has ever been discovered between the peritoneum and these organs. Appendices epiploicae are only found on the large intestine.

6. *Nerve Supply.*—Spinal sensori-motor and sympathetic nerves connect the visceral and parietal walls of the coelom to the spinal cord. It has been found expedient to preserve a direct communication between the lungs and the stomach, liver, spleen, and the medulla; hence the vagus, the nerve of the tenth segment, has followed these organs in their downward displacement, and connects them with important centres in the lower cerebrum.

7. *The Omentum.*

1. Its position: It covers like an apron every viscus in the cavity except the liver, stomach, and spleen.
2. Its fimbriated edge.
3. Its power of "movement."
4. Its disproportionate blood and lymph supply.
5. Its normal coating of two or three layers of leucocytes. Associated with this is its exemption from suppuration when it has been replaced after being herniated through a wound.
6. Its history: Found in mammalia and lower vertebrata; grows faster than other abdominal contents during life; before 45 above pubic spine, after that below it; left border lower than right.

The benefits which the human organism has been supposed hitherto to have derived from the existence of this organ are mechanical, and are, in my opinion,

* Read before the Gloucestershire Branch of the British Medical Association.

† I am aware that Cuneo in his recent work thinks that all the lymphatic vessels are closed.

secondary to its great function as the great scavenger of the peritoneal cavity. Barnard (*Contributions to Abdominal Surgery*, page 170) says:

> The functions of the omentum, so far considerable, are normal ones which must be served each day; but there is a function which is *seldom needed*, but which is so beautifully served that the omentum must be specially adapted to fulfil it. This function is to localize infections of the peritoneum. For this reason the omentum has been called the "sticking-plaster" of the peritoneum.

The fact that this function is so beautifully served is almost proof that the organ which performs it is in constant practice; the simile of the sticking-plaster is thus a mistaken one. I would compare the omentum to a minister for foreign affairs. However capable he may be, there are times when his country is embroiled with another (sticking-plaster period), but no one supposes that this is the only occasion on which he is serving his country. How many wars has he nipped in the bud by his successful and experienced diplomacy carried on over a long period of years, and of which the public are left, by his very success, in entire ignorance?

8. *Histology.*—The endothelial cells vary in size and shape in different parts of the coelom; some are columnar, over the ovary; some large and transparent; some small, coarse, and granular; some kidney shaped; in the lower vertebrata some may be ciliated. It is reasonable to assume that these cells are capable of different functions.

The Development of the Coelom.

In animal life its appearance is next in antiquity to that of the archenteron or primitive alimentary canal. The coelom, therefore, appears correspondingly early in the human embryo.

When the organism consisted only of an outer layer, *ectoderm*, protective, sensitive and reproductive, and an inner, *endoderm*, digestive and absorptive, the even distribution of nutritive material was a simple matter, even when the ectoderm was several cells thick. When, however, a third layer, *mesoderm*, was developed primarily for reproduction, secondarily for support and locomotion, any further progress in usefulness of this layer would be considerably hampered if its proper supply of nutriment continued to depend upon a correspondingly complex evagination of the alimentary canal amongst its cells. This difficulty would be solved by cleavage of the mesoderm into an inner thin layer, the splancheropleure, and an outer layer, the somatopleure with a cavity (the coelom) containing fluid kept in motion by the movements of the organism. Not only did this cavity serve as a *nutriment distributor*, but it also became a receptacle for the *products of metabolism* of the cells: the cavity was therefore placed in direct communication with the exterior. In addition, the *reproductive cells* were shed into its interior, and thus found an easy exit. These three uses alone would abundantly justify, by their unquestionable utility, this departure in the evolution of animal life.

In the trochosphere, the larva of the *Polygordius* (Archiannelida), we have evidence of the utility of a cavity between the ectoderm and the endoderm when none of the above three functions are required of it, for the ectoderm consists of a thin layer of cells, and could be adequately nourished by direct contact with the cells of the endoderm, for the mesoderm is only rudimentarily developed at the hinder end of the organism; the products of metabolism could be got rid of by the same method as in Coelenterates. It is certain also that the cavity is not a receptacle for reproductive cells. It is large in comparison with the size of the organism, and is in direct communication with the exterior by means of the pronephridia. It is by no means the homologue of the coelom, being, in fact, the blastula.

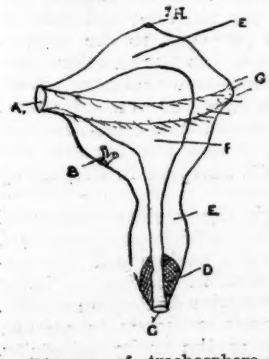


Diagram of trochosphere. A, Mouth; B, pronephridium; C, anus; D, mesodermic bands; EE, perienteric cavity; F, stomach; G, cilia; H, eye.

I may assume that along with the food a large number of cocci, bacteria, moulds, protozoa, etc., would pass into the alimentary canal of this organism, some of them actively hostile and pathogenic (to it). With a large number of these the cells of the endoderm would be able to deal, but it can be imagined that from the number or virulence of the invading organisms the resistance of this first line of defence would be, not infrequently, overcome—too frequently for the successful propagation of the species. It is important that the contents of the canal should be freed from this contamination before it reaches the hinder end of the alimentary canal, for here, surrounding the endoderm, are the mesodermic bands which contain the elements essential not only for the development of the adult annelid, but for the development of countless numbers of annelids in the form of reproductive cells. Any bacteria, therefore, which succeed in defeating or eluding the endodermic cells only find themselves in a lethal chamber containing lethal fluid, and such again as are able to survive the effect of this fluid would be swept out through the pronephridia.

I have long thought it singular that, whilst incalculable time has been devoted to discovering the various mechanisms for digestion and assimilation, and in describing and exhibiting the admirable and beautiful methods of defence against what I call the "macroscopic" enemy, very little has been expended upon elucidating the methods of defence of the single strong, complex organism against the battalions of insidious microscopic low-organized enemies which it does, indeed it must, admit into its alimentary canal. This struggle is incessant, and the life of the host depends upon a long-sustained and continual victory; it has been going on from time immemorial, from the beginnings of animal life, and the progressive evolution of animal life only became possible by the establishment of a sound scheme of defence against them. In protozoa and diploblastic metazoa (Coelenterates) the defence is a simple matter. The development of a third layer and its progressive increase in bulk and complexity caused too much to depend upon the impregnability of the endodermic epithelium. The coelom was therefore developed between the enteron and the rest of the body, and it is on the other side of the coelom that animal life has developed its wonderful complexity and adaptability. The enteric side of the coelom is as simple almost in us as it is in the snail. The duty of the coelom was to protect the other side from the entrance of any foreign organism. It is, then, only the development of the coelom which has rendered possible the evolution of higher organized life.

In the adult *Polygordius* this cavity is retained as the cavity of the peristomium, and a coelom is developed from the mesoblastic bands; the cavities in the metameres of the adult worm separating the alimentary canal from the body wall are directly formed by the cleavage of the mesoblastic bands at the posterior end of the trochosphere. For the sake of clearness I will drop the ancestral *Polygordius* and take up its near descendant, the earthworm, which lie in the main channel of evolution from the protozoon to the craniate vertebrate.

I commenced with the assumption that the coelom was originally designed for three purposes:

1. The formation and collection of reproductive cells ready for shedding upon the exterior at the opportune moment.
2. The even distribution of nutritive material.
3. As a receptacle for the products of metabolism.

The plan of the trochosphere suggested the possibility of another function. I am confirmed in this belief on examination of the coelom of the earthworm, for we find:

1. The formation and collection of reproductive cells has been relegated to one or two segments, yet the cavity persists in all the segments except the first few.

2 and 3. An elaborate blood circulatory system has been formed which ensures: (*a*) That nutriment is conveyed from the chloragogen cells to the rest of the body; (*b*) that oxygen is conveyed from the skin vessels to the rest of the tissues and the carbon dioxide from the tissues to the skin; (*c*) that the products of metabolism are conveyed from the tissues to the nephridial tubes and thence to the exterior.

An explanation is required as to why:

(*a*) The coelomic cavity is relatively so large; a characteristic of the vermes from the platyhelminth to the choetopod.

(*b*) The use of the dorsal pores, for which I have found no suggestion; and further, why in the ancestral *Polygordius*

these are found in every segment, whilst in the earthworm they are not found until the eighth.

(c) Why, in spite of the existence of a circulatory system, the nephridia still open by bell-shaped ciliated mouths directly into the coelomic cavity.

To take the last first, it might be urged that the organism did not as yet depend upon the circulation entirely for the conveyance of metabolic products to the cells of the nephridial tubules, and that even if it did, as yet the funnels had not had time to degenerate; to which my reply is that tubules in direct communication with the exterior open by funnel-shaped mouths into the coeloms of the various orders right up to and including vertebrate fishes, even amphibia. It is significant that in the latter these are mesoblastic and not ectoblastic, so that when the disappearing ectoblastic nephridium is replaced by the mesoblastic tubule of the pronephros, and this in spite of improved elaboration of the circulation, I am entitled to gather therefrom that the coelomic fluid contains material which is better conveyed directly to the purifying cells of the tubules than through the systemic circulation. In the case of the earthworm it might still be true that a large part of the products of body metabolism were thrown directly into the coelom and then extracted by the tubules, but in the case of the lower vertebrates this could not possibly hold good, as only an infinitesimal quantity of these could be cast directly into the coelom. At the least it may be assumed that it is a similar necessity which has to be met by organs which resemble one another so closely in site and structure yet morphologically are so different. I believe that the coelomic fluid is poisoned by the toxins of bacteria, etc., which have entered through the walls of the alimentary canal; that the contaminated fluid is guided down the funnels by the cilia; that the water, or part of it, is reabsorbed by the cells of the tubules back into the general circulation, only sufficient being allowed to pass down the tubules to wash the toxins out to the exterior. In this way the coelomic fluid is kept clean of its poisons without risk to the body tissues.

The earthworm obtains its nutriment by passing immense quantities of decaying and organic matter and soil through its alimentary tract. Darwin has pointed out that earthworms are an important agent in the formation of vegetable mould. I venture to add, in its purification also. The food taken in by the mouth literally teems with bacteria of all sorts and kinds. Are we to believe that the only defence against the entrance of these into the body tissues is a single layer of columnar ciliated epithelium and a few layers of chloragogen cells? If that were so every earthworm would find death or disease the inevitable sequel of its partiality for what the gardener calls "fat stuff," and as organisms of use to the community they would cease to exist. I believe the bacteria, etc., normally pass in their myriads, some killed, some of them maimed, perhaps to some extent, after their struggle with the first line of defence, into the coelomic cavity, where they are effectually dealt with by the amoeboid cells and the bactericidal fluid of the coelom. The particulate relics of the encounter of both sides are swept out through the dorsal pore, not down the nephridial tube, the mouth of which is guarded by the cilia, which prevent the blocking up by such particles of the delicate, slender tubule, whilst the fluid itself is relieved of its contaminating toxins in the way just mentioned. This explains the large size of the coelomic cavity and the presence of the dorsal pore behind the pharyngeal segments, which, owing to the thickness of the pharyngeal wall, would not require one. It also explains how the soil is effectively purified of deleterious organisms, or, rather, how their excess is regulated.

I do not intend to give detailed descriptions of the internal organization of the echinoderms, arthropods, gastropods, brachiopods, molluscs, and tunicata. It will be sufficient for the present discussion if I point out that in all the absorptive part of the enteron runs through a cavity which either

(1) Communicates directly with the exterior via generative opening, or the renopericardial opening and the ureter if the cavity is the whole or part of the coelom.

Or (2) if it is not the original coelom, is its derivative and part of the blood vascular system, the perienteric cavity communicates by large and direct channels with the gills or other respiratory organ or immediately underlies a large respiratory chamber, and thus the fluid in the cavity is in great part only separated from the exterior by a very thin layer of cells or delicate transparent membrane which would readily permit the excretion of toxins or the egress of amoeboid corpuscles containing organisms.

To my mind it is no objection to this general formula that in some of the above orders, perhaps most, the perienteric cavity is not the coelom but part of the blood vascular system. Rather is it an argument in my favour that, in spite of, let us suppose, the degeneration of the coelum, or perhaps because of it, a large perienteric chamber was developed round the enteron because it was an absolute necessity to the organism.

I will now examine a more direct descendant of the earthworm, the *Amphioxus*. In this acraniate vertebrate we find the same arrangement precisely with the added

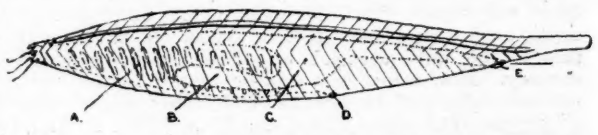


Diagram of *Amphioxus*, to show relation of stomach and hepatic caecum to atrial cavity. A, Gills opening from pharynx into atrial cavity; B, hepatic caecum; C, stomach; D, atriopore; E, anus.

certainty that the perienteric cavity is really a coelom. This cavity is in direct communication with the exterior, not by one but by many tubes which open at the top of the branchial chamber on each side. I attach some importance to the fact that the perienteric cavity is in reality the coelom and not a secondary derivative, for the coelom is formed in this and probably in all vertebrate descendants by a process of budding from the wall of the enteron; the cavities of these buds run into one another to form the coelom, whilst the walls form the mesoderm and become the splanchnopleure and the parietopleure.

The cells of the enteron in Coelenterates are capable of seizing upon and digesting bacteria in the food, hence the coelomic epithelium would, as it were, have a hereditary instinct for slaying and devouring harmful organisms.

The short tubules of the *Amphioxus* give place to the pronephric ducts and tubules in the lower craniate vertebrates, these again to the Wolffian ducts and tubules and the Müllerian ducts. In addition some reptiles and fishes have "coelomic canals," short direct communications between the coelom and the external medium. For the latter no use has apparently been found. As for the former, they undoubtedly act as excretory ducts for the waste products and generative cells. But if this constitutes the whole of their function then certain difficulties must be satisfactorily explained:

1. The number of Wolffian tubules relegated to the duty of conveying the male generative products forms a very small proportion to the total, and in most orders these few are in direct communication with the testes. What is the function of the remaining large majority?

2. The waste products of metabolism are conveyed to the above ducts and tubules by renal, portal, and other vessels; there is, therefore, no apparent necessity for these tubules to open directly into the peritoneal cavity.

I contend that these openings are retained because the coelom contains poisons which are not absorbed into the general circulatory system, but conveyed to the tubules through the open funnels and so out to the exterior; that these poisons are produced by bacteria which have passed through the intestinal wall. In times of unusual stress, during an abnormally large permeation of organisms, the coelomic canals are made use of and the bacteria and toxins washed out by the serum secreted in defence by the peritoneum.

Now, as we proceed through the various orders of vertebrates, from the acraniate to the highest mammalian, direct communication between the coelom and the exterior gradually disappears, so that in the human being the only direct channel is the Müllerian duct of the female. Although I am not going to exclude the duct from any possible share in the cleansing of the peritoneal cavity, still to maintain my theory that the peritoneum is a constantly infected cavity it is necessary that I find some other excretory channels to replace the more direct ones. It would be in the highest degree improbable that the direct apparatus should be comparatively suddenly discarded and another indirect one equally suddenly brought into play. It is more likely that the new methods had

been for thousands of years in some similar sort of use, and that the convenience of the organism required or its environment enabled it to depend more and more upon the indirect channels, which, as the demand made upon them became greater, became more perfectly adapted to their work.

I believe I find both methods in use in the lowest vertebrate, the *Amphioxus*. When we compare this animal with the earthworm, where we found the direct channels at their best, we notice that:

1. Whilst the rest of the animal is markedly segmented, the coelomic segments have been run into one cavity and its two anterior prolongations, the dorsal coelomic canals. The excretory tubules, of Lankester and of Weiss and Boveri, are situated in these prolongations.
2. The absorptive part of the enteron is dilated and sends a prolongation forwards by the side of the pharynx (the hepatic caecum) covered by two layers of coelomic epithelium with space between and the somatopleure. Both the dilated enteron and its pouch are placed within the atrial cavity (the great gill chamber), where water is in constant circulation, where the somatopleure can afford to be very thin, as it is still protected by the walls of the atrial cavity, the metapleural folds. Behind the atrial pore the enteron is narrow and short, the somatopleure is comparatively thick, but the coelomic cavity is wider and better marked here than anywhere else.

Now, what benefits does the *Amphioxus* derive from this arrangement? From the point of view of digestion and absorption I answer: Digestion and absorption could be equally well carried on if the dilated enteron and its pouch, the hepatic caecum, were placed behind the atrial cavity or entirely excluded from it. The thickness of the somatopleure would be a matter of no concern. So that whatever functions this arrangement subserves, I think we can confidently say they are not connected with the assimilation and absorption of food. My view is that pouching and dilatation of the enteron, being designed for the stagnation of the foodstuffs, carries with it the disadvantage that, owing to the bulkiness of the mass of food, the bacteria which swarm in it have increased opportunity for multiplying and, owing to the stagnation of the food, these increased numbers have increased opportunities for overcoming the resistance of the single layer of epithelium and passing through into the coelom, which is here very narrow; in fact, the two layers of coelomic epithelium are almost in apposition. This enables the parietal epithelium to pick out the toxins into the lymph space of the somatopleure, which is here exceedingly thin, so that they are again immediately picked out by the atrial (dermal) epithelium, and so excreted into the gill cavity, whence they are washed out by the constantly circulating water. Any bacteria which forced their way through would meet with the same fate, though I think that the greater number of these would be washed out through the nephridial tubules, for the fluid in the coelomic cavity would be in constant motion. Now it is plain why the dilated part of the intestine and its diverticulum are placed in that part of the coelom within the atrial cavity, for behind the atrial pore, the somatopleure being of much greater thickness, and containing much muscular tissue, toxins and bacteria, once they gained admission into it, would be able to enter the general circulation.

But any excretion here is unnecessary, for the part of the alimentary canal posterior to the atriopore is narrow, and practically only a duct to the exterior, whilst the coelomic cavity is conspicuous. Have I made too large a demand in assuming that the coelomic epithelium can pick out toxins and bacteria? We see this power in its correlated descendant, the lymphatic and blood capillary endothelium, for it is only owing to the selective power of this cell that the different tissues are able to obtain the substances they require. This cell is also able to pick bacteria out of the blood and lymph, so that I am not ascribing any very great property to the coelomic cell, its ancestor. Once in the lymph of the somatopleure, here exceedingly thin, the atrial epithelial cell excretes it on to the exterior, and it is washed by the current of water out of the branchial chamber. I have no proof that the atrial, that is, epidermal, epithelium possesses this property, except to point to the great powers our own epidermal cells have in this respect, for under stress they will even excrete urea, etc., and other noxious products of the body metabolism.

I believe likewise that in the human organism bacteria escape in swarms from the canal, past the intestinal epithelium into the submucous tissue lymphatic spaces. There the leucocytes are waiting for them, and no doubt account for a good proportion of them. There are, however, many kinds of bacteria, and unfortunately they are the most virulent, which leucocytes are disinclined to interfere with. These pass into the thickness of the intestinal wall until they come to the muscularis mucosae; this acts as a close-meshed net, which, whilst it will not bar their further progress in twos or threes, as it were, bars the further progress of a clump which would have a great power for evil, and causes them to spread out into a thin layer. The thinned-out line passes still further into the interior until it meets the layers of longitudinal and circular muscle fibre, where a further spreading-out process takes place until, by the time they reach the lethal peritoneal epithelium and the omentum, the original clump is spread out over a considerable surface and makes the work of these cells considerably easier. Should they survive the coelomic epithelium of the splanchnopleure their further wanderings are ended by the layers of leucocytes and the quadruple layer of coelomic epithelium of the omentum, which lies like a huge parasite dining off a constant meal of organisms which it does not even require to go in search of, for the vermicular and peristaltic action of the intestines renders this unnecessary. The process which I have just described may be seen in the spread of small tubercles on the serous surface round a tuberculous ulcer of the intestine. The area would have been larger and their number would have been much greater but that the "outliers" were never in sufficient strength to resist the omental and peritoneal epithelium and the omental leucocytes.

It may be asked why they do not enter the blood and lymph capillaries, and thus get into the general circulation. Now, the region in which they are most likely to do this is the submucous tissue; their entrance into the lymph vessels is prevented by the fact that here, unlike any other region of the body, the lymph capillary (or lacteal) does not begin in an irregular open lymphatic space, but has a club-shaped end closed in by cells which allow the passage inwards of leucocytes containing fat, but have the power of excluding undesirable passengers—the endothelium of the blood capillary has a similar property of exclusion and selection, in addition to which, in this region, this defence is aided by the immense number of leucocytes present. I take it that the immense numbers of leucocytes which are present on the hypoblastic portion of the gills of fishes are there, not to nip up bacteria escaping from the blood capillary, but rather to prevent any undesirable micro-organism settling upon and then invading the delicate epithelium of the blood capillary, here part of the systemic circulation. Here virulence of the micro-organism does not matter, for the leucocytes are constantly shedding themselves and their foe into the gill stream. In the lymphoid tissue of the intestine virulence of the invader does count, but though the leucocytes apparently shrink from tackling him, as they do the less virulent—that is, by engorging—still they drive the invader into the far more fatal trap further in—that is, the coelom.

But in spite of all this, micro-organisms do manage to gain entrance into the blood and lymph capillaries.

The lymph capillary is more likely to be invaded than the blood. They are still liable to be picked out again by the endothelial cells, and any which may escape these, in the case of the lymph capillary, meet their fate in the mesenteric glands, and, in the case of the blood, in the liver, where they are arrested by the pseudopodia of the liver cells bordering the portal capillary (Werigo). Thus is the lymph purified before it reaches the receptaculum chyli and the blood before it reaches the hepatic veins.

Now we have to consider the methods by which the peritoneal "shambles" is cleared of the bacterial carcasses and their *ante-mortem* toxins. We have examined them in the case of the *Amphioxus*. Man is, however, at a disadvantage as compared with the lower vertebrates in having no direct outlet to the exterior.

At times of small or normal stress—that is, when no "symptoms are produced"—the bacteria (dead) are conveyed by.

1. Leucocytes, where they may be digested entirely.
2. Leucocytes, back into the intestine, and thus to the exterior.
3. Leucocytes into (*a*) mesenteric glands by the lymph channels, (*b*) liver by the portal blood of omentum and intestine, where they are arrested by the liver cells and cast out into the bile.

The contaminated lymph of the cavity is—

1. Excreted by the visceral endothelium back into the intestinal lymph, whence it is (*a*) extracted by Lieberkühn's glands into intestine; (*b*) absorbed by veins and extracted, and perhaps changed by liver cells and thrown into the bile which passes down the intestine, and acts possibly as an antitoxin to that particular variety of bacteria.
2. Absorbed by omentum, and thence into liver, etc.

How often has it happened that a surgeon, when operating for a totally different abdominal condition, has found miliary tuberculosis of the peritoneum which has given rise to no "symptoms"! I had such an experience a short time ago. I was operating for a ventral hernia, and, to my astonishment and dismay, discovered that the peritoneum—parietal as well as visceral—was covered with little millet-seed tubercles. The case did perfectly well. Now, if I had not operated, this incident in the patient's history would never have been known, for, the normal machinery being equal to the demand, no physical signs or symptoms were produced.

I would also include here the thousands of abdominal cases which in the pre-antiseptic and even the pre-glove days emerged successfully from a surgical operation, with suppuration limited to the abdominal wall. The peritoneal cavity had done its work silently and effectively. Lane's constipation bands are the scars of silent battles in the peritoneum.

Now let us take a case in which the stress upon the peritoneum was so great that the normal machinery I have mentioned is not equal to the task imposed upon it. Nature is still far from the end of her resources; but, from the very fact that unusual methods of dealing with it are called upon, "symptoms" and physical signs are produced. These, so far from making us believe that physiology is "disordered" (Barnard), ought plainly to point out to us the extra reaction to an extra injury. Pathology is, in fact, not "disordered physiology," but "extraordinary" physiology.

The symptoms and physical signs of acute miliary tuberculosis which has been caused by the rupture of a mesenteric gland, Fallopian tube, or from a tuberculous ulcer of the intestine, are clues to the extra channels of excretion made use of in times of great stress upon the peritoneal cavity.

To begin with, the invasion is not by permeation, but by sudden flooding from a diseased focus, by bacilli and their toxins; hence there is pain; the overdose is thinned and spread out as rapidly as possible, a large quantity of bactericidal serum is secreted; hence there is ascites, which is not only fatal to the bacilli, but dilutes their toxins. Some of the toxin escapes into the systemic circulation; hence there is rise of temperature and rapid pulse. In order to limit the amount of toxins escaping into the general system, in spite of the arterial hyperaemia, which is necessary, the blood to the liver is stagnated as much as possible in order that the liver cells may have a fair chance to cope with it; hence the intestine has a congested appearance, and its movement is reduced to a minimum, and even inhibited entirely, and so we find constipation; in addition to which the intestine, *pro tem.*, acts as a respiratory chamber for the reception of the CO_2 and the waste products of the venous blood; hence there is tympanites. Were it not for this temporary relief the vitality of the bowel wall would be very seriously and rapidly interfered with. Under these circumstances, it can be imagined that the intestinal route for the excretion of the toxins from the peritoneal cavity would not be a very efficient one, so another has to be found. The peritoneum of the stomach now takes on the work; the peptic glands do the work normally done by Lieberkühn's glands. The bowel being paralysed, a new duct to the exterior has to be found. The stomach is filled, not only with the toxins excreted by its own glands, but also with the excretions of the liver, and the vomiting centre in the medulla, being stimulated by toxins which have gained access to the systemic circulation, ensures (via the vagus?) a periodical expulsion of its noxious contents; hence the vomiting of large quantities of fluid. The colour of the vomit will depend upon the efforts which the liver cells have to make to meet the difficulties of the case. If the stress is small the hepatic centre in the medulla which regulates the metabolic activity of the liver cells (Bernard's glycogenic centre) is stimulated by poison in the systemic circulation to a corresponding effort only; hence the vomit, after the expulsion of food, will contain light yellow bile and the acid secretions of the stomach; under greater stress the latter will be mixed with light green bile, and so on, until, in virulent septic cases, such as peritonitis from a perforated gangrenous appendix, we see it gradually change to sage green (acid), dark spinach green (acid), to black and so-called "faecal" (alkaline towards the end).

Such, then, are the "local" symptoms of acute tuberculous peritonitis. If the dose is not too large, with the aid of this extra help the peritoneum recovers itself, shuts up the bacteria in tubercles and deals with them leisurely in the way in which I have before suggested. The omentum shows the most severe signs of the past struggle. As there is considerable leeway to make up, diarrhoea sets in; the ascitic fluid is gradually thus drained away, the temperature and pulse become lower, and the balance of health is restored. An abdominal incision expedites matters considerably.

If the original dose of bacilli is overwhelming, or, as in the case of tuberculous ulceration of the intestine, not only overwhelming but continuous, the bacilli cannot be sufficiently rapidly thinned and spread out, the result being the caseous or fibro-caseous peritonitis which even surgical assistance is so powerless to help.

The peritoneum may be invaded by all kinds of bacteria and cocci of all grades of virulence, either gradually, as through an inflamed appendix, infected Fallopian tube, the weakened intestinal wall behind a growth or other non-acute obstruction, etc., or overwhelmingly, as through a perforated gangrenous appendix, gastric or duodenal ulcer, from extensive ulcers in the intestine, strangulated bowel, or through bowel wall behind a chronic obstruction, whose vitality has at last been so diminished as to offer no resistance to the passage of bacteria from the intestine into the peritoneum (so-called "acute obstruction"), or from the extensive surface of a virulently infected puerperal uterus. The symptoms and physical signs of the peritoneal toxaemia will vary not only in intensity but according to the particular channels by which the peritoneum attempts to relieve itself—for example, diarrhoea is more characteristic of puerperal fever and pneumococcal infection than others, and vomiting, therefore, less conspicuous.

The greater the virulence of the toxins, the more the the pain and the more Nature tries to limit the poison to the peritoneal cavity; hence there is more rigidity, constipation is more absolute, the vomiting more incessant (with exceptions where the ratio is inverted). There is more tympanites. The constitutional symptoms caused by the systemic poisoning are more marked, and all possible channels are used—for example, the lungs and pleurae, as shown by the frequent and shallow respiration, the peripheral arteries are constricted to minimize the poisoning of the tissues and increase the supply to the visceral vessels and kidneys; the extremities become cold; the sweat glands are called upon to join in the work of excretion in order to stave off as long as possible, and perhaps prevent, the final paralysis of the heart. The tissues are drained of fluid in order to supply the peritoneal cavity by the visceral arteries, with a wash-out which passes out through the liver and stomach.

The prevailing theory of the causation and mechanics of faecal vomiting (Haguenot's) is entirely wrong and is open to insuperable objections (see Hemmerden, II, 157, for a statement of this theory).

1. It occurs when there has been *no* intestinal occlusion, such as very bad cases of appendicitis, and impacted gall stone in *cystic* duct, and acute pancreatitis, cirrhosis of liver (terminal stage).
2. The faecal vomit is not the same in colour as the contents of the higher reaches of the intestine, as was proved in a case of impacted gall stone in the jejunum which I operated upon a little time ago.
3. Even in cases of intestinal obstruction it appears com-

paratively late, the various vomits following a definite sequence of which faecal vomit is the last and comparatively late stage, except in impacted gall stone, whether in the cystic duct or intestine, when it appears very early.

4. The amount vomited is far greater than the amount taken in.

5. In cases of intestinal occlusion—say, of annular stricture of the small intestine—the more the faecal vomiting, the more relieved the patient should feel. On the contrary, the more toxic he appears.

6. In cases that recover there is an intense diarrhoea at first of small quantity of dirty brown material, then of large amount of light liquid, lemon-yellow faeces, showing that the liver cells have not regained their normal powers.

7. If the faecal vomiting occurs sometime subsequent to the insertion of a drainage tube in the peritoneal cases—that is, non-occlusion cases—then the patient will probably recover; whereas if operation is deferred until it has occurred the case is hopeless. An exception must be made in the case of impacted gall stone, as faecal vomiting is so often an early symptom, and followed by temporary relief.

8. A most fatal objection is that the higher up the obstruction the more profuse is the vomiting. The contrary should be the case. My explanation of this phenomenon is that the number of bacilli which should have been spread over the whole length of small intestine now flood the upper reaches of peritoneum, and stress is more rapid and urgent on stomach and liver: the need being great early, the liver early puts forth all its strength.

The reaction and colour of the vomit have, to me, become important indications, not only in the diagnosis, but still more in the prognosis of inflammatory conditions inside the abdomen.

For instance, if the case has been ill for more than three or four days and the vomit is still yellow and acid, I know there is no perforation or acute obstructive condition, but simply inflammatory, and if surgical interference is necessary, the prognosis is good. On the other hand, if by this time the vomit is black or coffee-coloured, the diagnosis is that of acute virulent, thin turbid fluid peritonitis—for example, pneumococcal (rare and fatal cases), or perforative gangrenous lesions of some part of the intestinal tract, and if surgical aid has not already been given the case is absolutely hopeless. A dark, spinach-green vomit before operation makes the prognosis exceedingly grave. A home-made beef-tea-like vomit generally indicates pus and a certain amount of lymph, and one sees it in some cases of gonorrhoeal peritonitis and general purulent peritonitis from appendicitis. Operation, to have any hope of success, should be undertaken instantly. The prognosis is bad. Early faecal vomiting is a certain sign of impacted gall stone, and prognosis, if operated upon, is good. Late alkaline faecal vomiting means acute strangulation, acute gangrenous appendicitis, etc., and operation, if it has not been previously undertaken, is now out of the question. If it comes on after operation, is acid or neutral, with clean tongue, even though there still be absolute constipation and great tympanites, if the pulse be under 100, the prognosis is quite good.

In operating on septic abdominal cases it should be remembered that the liver and stomach, if chloroform is given, have to excrete this drug from the circulation as well. The double duty handicaps them. Ether is better in such cases.

It is not necessary to enter upon a detailed explanation of the pathology, physical signs and symptoms of a midway type, such as pneumococcal peritonitis, chronic proliferative peritonitis, etc. The variation in the reaction of the peritoneum is due to the difference in the virulence of the organisms permeating the intestinal wall.

Localized peritonitis—for example, round an appendix—should present no difficulties in the way of explaining both its method of localization and why its symptoms are those of a general peritonitis. The vomiting in these cases is said to be reflex. I claim that my theory is the more reasonable. Does it not also throw light on the symptoms of "appendix dyspepsia"?

A boy's bilious attack is a peritoneal toxaemia; so also are the summer diarrhoea of children and Asiatic cholera—that is, the peritoneal cavity is the seat of the elaboration of the poison by the bacteria.

Some of the cases of "vicious circle" after gastro-jejunostomy are instances of septic peritonitis. Some of the failures of gastro-jejunostomy are due to the fact that the operation has been performed, not on account of pyloric obstruction or gastric or duodenal ulcer, but in cases of chronic infection of the peritoneal cavity, and the seat of this was not found; the poisoning, therefore, continuing, the liver and stomach continued their necessary work of excretion.

Take, again, some—I do not say all—cases of chronic atonic dilatation of the stomach (without obstruction). I believe the cause of the condition to be due to the following sequences of events:

Abnormal microbes in excessive numbers in the intestine (due to unsuitability of, and over-indulgence in, food and drink), permeation of these into peritoneal cavity; there being constipation, the stomach assists the liver in the work of excretion, and therefore constantly contains toxic secretion; this duty and digestion cannot both be performed properly, the former, being of the most vital necessity, is proceeded with; food therefore ferments, the numbers of the bacteria are thus increased, and a vicious circle set up: the muscle coats are constantly soaked in toxic lymph and becomic atonic, etc.

I have a desire to believe that gastric ulcer has somewhat of the same causation, and that, in addition to toxins, bacteria themselves are thus excreted. Some of these bacteria, during the time that they find themselves in the submucous plexus, begin the mischief of ulceration, but I I merely whisper the suggestion at present.

The symptoms of acute intestinal obstruction, as given in the textbooks, are not symptoms of obstruction at all, but of the supervening acute virulent peritonitis due to the excessive permeation of virulent micro-organisms through the bowel wall. Such a permeation may take place without obstruction, and Hemmeter says that "many cases have died with the symptom of acute intestinal obstruction where none could be found at the *post-mortem* examination."

TUBERCULOSIS IN INFANCY AND CHILDHOOD.

FROM THE STANDPOINT OF PREVENTIVE MEDICINE.

BY

CHARLES McNEIL, M.A., M.D., M.R.C.P.EDIN.,

EXTRA PHYSICIAN, ROYAL EDINBURGH HOSPITAL FOR SICK CHILDREN; ASSISTANT IN PATHOLOGY, UNIVERSITY OF EDINBURGH.

IN the JOURNAL during 1910 an article by Hamburger appeared, setting forth the results obtained by him in Vienna by the use of the cutaneous tuberculin reaction in a large number of apparently healthy children of all ages from infancy to puberty.[1] These results were remarkable, for they showed that tuberculous infection, entirely, of course, in a latent form, and beyond the reach of the most refined physical examination, began to occur in infancy, and so rapidly and steadily increased during childhood, that by puberty about 90 per cent. of poor children in Vienna reacted positively to the tuberculin test. Von Pirquet's classical investigation, published in 1907, had demonstrated for Vienna the same striking fact—namely, the existence in childhood of a widespread latent tuberculosis, in addition to the mass of active and clinically apparent tuberculosis.[2]

The following is a series of 371 cases in which cutaneous tuberculin tests were performed on in-patients of the Royal Edinburgh Hospital for Sick Children; they make it possible to institute a comparison between Edinburgh and Vienna. Both von Pirquet's results and my own included cases of active and recognized tuberculous disease, and are therefore strictly comparable; while Hamburger's figures, showing only latent or concealed tuberculosis, must be kept separate.

The Edinburgh cases were not selected in any way, but were taken systematically from all the medical wards of the hospital. The series was not consecutive, being interrupted from time to time. It includes a number of clinically tuberculous cases, but no attempt was made to secure a large number of these, or of any other clinical condition. The series, therefore, forms a representative group of the in-patients of this hospital, drawn from the poorer classes of the population of Edinburgh. The few cases from rural districts will not materially injure the deductions, and such cases are no doubt also included in the Vienna figures. The cases are arranged in six age periods, similar for both places. In the Edinburgh table there are no cases over 12 years.

Age Period.	Edinburgh.		Vienna.	
	Number of Cases.	Percentage of Positive Reactions.	Number of Cases.	Percentage of Positive Reactions.
Under 1 year	64	14.1	156	5.1
1-2 years ...	61	29.9	98	22.4
3-4 years ...	75	46.6	108	37.9
5-6 years ...	52	28.8	85	62.3
7-10 years ...	79	51.9	119	59.6
11-14 years ...	40	55.0	65	73.8
Total ...	371	37.7	631	38.5

These results can be conveniently shown and compared by means of graphic curves, as in the accompanying chart:

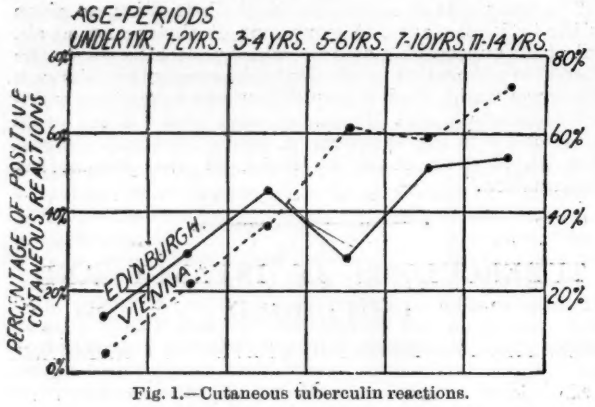


Fig. 1.—Cutaneous tuberculin reactions.

The two curves for Edinburgh and Vienna agree in some respects and differ in others. In both towns tuberculous infection begins in the first year of life, and increasing steadily in the successive stages, is found to prevail in a high degree about the age of puberty. But there are some differences. The curve for Edinburgh rises more steeply at first, and almost attains its maximum at the age period 3-4 years; after a drop in the next period (possibly to be accounted owing to the small number of cases), it rises again by smaller increments. The curve for Vienna is more uniform in its progress throughout, being distinctly lower than that of Edinburgh in the first three age periods, but overtaking it and decisively surmounting it in the latter half of the chart. In trying to interpret these curves from the standpoint of the sources of infection, it may be possible to show that this different behaviour of the two curves is significant.

It is generally agreed that, for practical purposes, there are only two sources of infection in human tuberculosis—the disease pulmonary phthisis in man conveyed by sputum and disease in cattle conveyed in tuberculous milk. It is seldom possible to indicate on a large scale the amount which each of these sources of infection, human and bovine, is contributing to the total of human tuberculous disease; but in the case of Edinburgh and Vienna the figures of tuberculous infection in childhood already given, along with certain other data, can be made to furnish a rough estimate of the respective parts played by the two factors in this widespread infection of the child population with tuberculosis.

The amount of human infectious material in a locality may be fairly gauged by the prevalence of phthisis within it, and so by the phthisis death-rate. The following figures give for Edinburgh (1909) and for Vienna (1906) the number of deaths from phthisis per 100,000 of the population. The figures for Vienna were kindly obtained for me by Dr. J. C. Dunlop, Superintendent of Medical Statistics, Register House, Edinburgh:

Edinburgh, 117 Vienna, 293

In Edinburgh, therefore, the amount of human tuberculous material of an infectious kind is little more than a third of that in Vienna. Further, Edinburgh has possessed since 1887 in the Royal Victoria Dispensary for Tuberculosis an organization specially directed to control and nullify this mass of infectious material; while Vienna, as far as I know, is without such an organized control. Yet in Edinburgh, with its much smaller phthisis death-rate, and with its special defences against human infection, the poor child-population for the first four years shows a greater degree of tuberculous infection than the corresponding population in Vienna, where the amount of human infectious disease is not only much larger, but is also less strictly controlled. There being only one other source of infection—the bovine—we are driven to conclude that the bovine factor must play in Edinburgh a larger part in child infection than in Vienna—in short, that in Edinburgh the children of the poor are seriously exposed to infection by tuberculous milk.

That conclusion is strongly supported by evidence of another kind. Abdominal tuberculosis, which especially prevails in the early years of childhood, is exceedingly common in Edinburgh; it is very uncommon in Vienna. Thomson and Fordyce[3] have prepared figures (from clinical records) showing the incidence of abdominal tuberculosis in various European and American hospitals; they give the following results for Edinburgh and Vienna, which endorse the above statement:

	No. of Cases.	Percentage of Abdominal Tuberculosis.
Edinburgh	15,320	3.6 per cent.
Vienna...	11,184	0.46 per cent.

It is not denied that abdominal tuberculosis may be caused by human contagion; but this factor may be expected to operate at least equally, probably more severely, in Vienna. As before, therefore, we are forced to the conclusion that bovine infection is a considerable factor in the tuberculous infection of Edinburgh children, and explains the higher curve in the early years of childhood as compared with that of Vienna.

From the standpoint of preventive medicine, the importance of being able to estimate, however roughly, the varying parts played by human and by bovine infection in this process of tuberculous inoculation is obvious. The statistics collected by Thomson and Fordyce, already quoted, are of great value for this purpose. They show that, while Edinburgh and Glasgow are easily first (from hospital clinical records) in the prevalence of abdominal tuberculosis, all the other British children's hospitals investigated show a much greater frequency of abdominal tuberculosis than do similar institutions in Italy, Austria, Germany, France, and America. The serious position which Great Britain occupies in this respect is graphically illustrated in the following chart, prepared from the figures of Thomson and Fordyce:

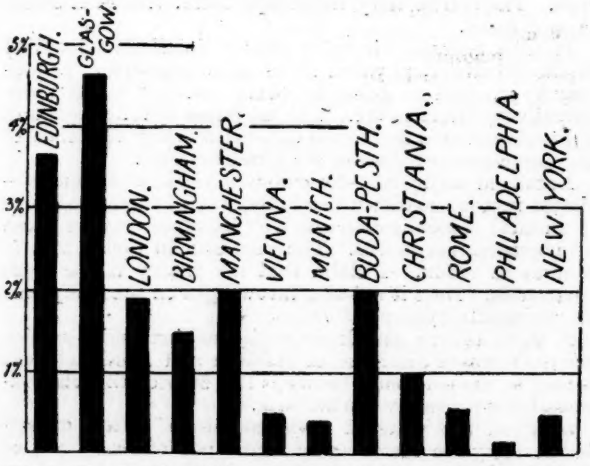


Fig. 2.—Showing the relative prevalence of abdominal tuberculosis in different children's hospitals.

I further endeavoured by the simultaneous use of human and bovine tuberculin for the cutaneous reaction to separate

cases of tuberculosis according to the source of infection, human or bovine.

Table showing Results in 330 Children, using alongside one another the Concentrated Tuberculin (Old) Human and Bovine, in each Case.

H = Human Tuberculin; B = Bovine.

H and B equal (both positive or both negative) ...	258
H and B both positive, but H greater than B ...	52
H and B both positive, but B greater than H ...	9
H positive, B negative	11
H negative, B positive	0

The results of this test defeated any hopes of discriminating in this way between a tuberculosis of human and one of bovine origin. They show that in the vast majority of cases, whatever the strain of bacillus present in the body, the reaction of the human tissues is the same, whether it is human or bovine tuberculin that is used to provoke it. It seems, in fact, another piece of evidence in favour of the view that human and bovine bacilli are closely kindred organisms. It is, of course, certain that some, and possibly many, of these 258 cases reacting equally to human and bovine tuberculin must have been bovine infections; but in the present state of knowledge this indifference of reaction cannot be used for or against the question of transmutation of the bovine into the human type of bacillus by residence in the human tissues. All that can be done is to state the results of this test and to leave them meantime unexplained.

So far, therefore, the conclusions are: (1) Tuberculosis, both as active tuberculous disease and as latent infection, is widespread among the poorer children of the Edinburgh population, and that by far the greater part of this is effected in the first few years of life.

(2) When one endeavours to fix the sources of this infection, whether derived from tuberculous disease in man or from tuberculous milk, by a comparison with conditions in Vienna, both as to tuberculosis in childhood and as to adult tuberculosis (phthisis), the conclusion is reached that bovine infection must have a considerable share in the tuberculous infection of Edinburgh children. This infection by tuberculous milk is chiefly operative in the first four years of life. It is specially marked in Edinburgh and Glasgow, but prevails in the large towns of Great Britain to a far greater extent than in most large centres in Europe and America.

(3) These facts of tuberculous infection in infancy and childhood, both as to its extent and its nature, must receive attention in any thorough scheme of administrative control of tuberculosis. They show clearly that the child is very susceptible to tuberculous infection, and that for Edinburgh at least this greater part of infection is effected *before school age.* This age incidence of the infection, if it can be shown to prevail generally, must make more difficult the problem of control. Some more effective measures for dealing with milk infection, which is a marked and special feature of tuberculosis in childhood in this country, are also called for.

It is very difficult to do more than guess at the relative parts played by human and bovine infection in Edinburgh. It is not meant to suggest that the factor of human infection is negligible. It is precisely during the first few years of life that the child is most dependent on the adult, and most closely in contact with h[illegible] most susceptible to human infection, and most difficult to guard from such infection. But these conditions are common to all countries and places, and they do not explain the greater incidence of tuberculosis of young children in Edinburgh as compared with Vienna.

Animal experiments, notably those of Findel[4] and Cobbett,[5] and those carried out by the British Royal Commission on Tuberculosis, have shown for various animals that it is more difficult to infect by swallowing tubercle bacilli in food than by breathing them in air. The flaw of all such experiments, used as applicable to human conditions, is that while this may be true of the healthy intestine, the same mucous surface in unhealthy conditions of catarrh, etc., may be easily penetrable by tubercle bacilli. In the frequent digestive disturbances of young children the intestinal ports of entrance, normally closed, may be thrown freely open.

The investigation by the British Royal Commission[6] of the characters of different viruses obtained from tuberculous disease in man is relevant to this point. In 29 cases of primary abdominal disease, by certain cultural and inoculation tests, 14 bovine strains of bacilli were isolated, and 2 of mixed human and bovine strains; the remainder were human strains. But in 11 of these 29 cases where the mesenteric glands alone were subjected to the tests, the results were 7 bovine and 4 human strains. Here again the possibility of the metamorphosis of types, and especially of the bovine to the human type, comes to the front. The Commission, after rigorous animal experiments, failed to obtain evidence that this transmutation of types takes place, but nevertheless concluded that "they are not prepared to deny that the transmutation of types does not occur in nature." In face of that conclusion, the separation of strains of tubercle bacilli according to the above cultural and biological tests does not give a decisive verdict on this point.

In any case this means of separation is not available clinically, nor is it practicable on a large scale from *post-mortem* material. For practical purposes a rough method of calculating the amount of human and bovine factors in the tuberculosis of childhood is to take, on the one side, the phthisis death-rate for the district, and allowing for any organized control of phthisical patients that exists, to regard its amount as measuring the human factor of infection; and, on the other side, to take the figures for abdominal tuberculosis as roughly indicative of the extent of the bovine factor of infection.

The Relation of Latent Tuberculosis in Childhood to Adult Tuberculosis.

In the 371 cutaneous tests performed on Edinburgh children, 297 were in cases that clinically were non-tuberculous. Of these, 28 per cent. reacted positively to tuberculin, indicating this proportion of latent as against active tuberculous infection. It is the presence of this large amount of latent tuberculosis that destroys the value of the von Pirquet reaction as a test of clinical tuberculosis. It is necessary to point out that the undiluted old tuberculin must be used if latent infections are being sought for. Mills,[7] in a recent analysis of the cutaneous test in 253 children, where he used a 20 per cent. dilution of old tuberculin, found negative results in 48 healthy children, whose ages ranged from 9 months to 12 years, and concluded that "a negative reaction may be confidently expected in children apparently non-tuberculous." I myself,[8] in a previous series published in the JOURNAL, and using a 25 per cent. dilution, obtained negative results in about 50 children suffering from diseases clinically non-tuberculous. But if undiluted tuberculin is used for the test, this statement is no longer true. Many apparently healthy children and many with diseases apparently non-tuberculous will then give a positive skin reaction.

The curves (Fig. 3) prepared from Hamburger's tables[9] give a picture of the frequency of latent tuberculosis at the various age periods of childhood. They represent

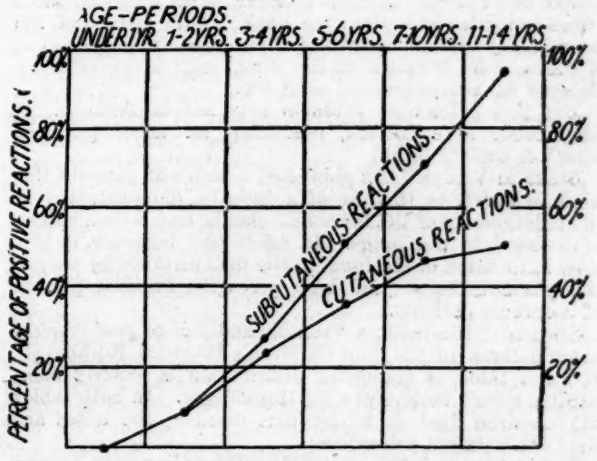


Fig. 3.—Latent tuberculosis (Hamburger), cutaneous reactions in 509 children clinically non-tuberculous.

tuberculin reactions given by 509 children, all of whom were by ordinary clinical tests free from tuberculous disease. The lower curve is of cutaneous reactions; the higher curve is of subcutaneous reactions (*Stichreaktion*), and represent positive results obtained by this more

delicate method in children who had failed to react to the cutaneous application of tuberculin.

This double curve of latent tuberculosis in childhood is interesting, for it serves to separate what may be called a latent infection of low potential from one of high potential. For the first four years of life the two curves are almost merged into one—that is to say, at this period all latent infections are of high potential; in other words, a quiescent focus may readily become an acute or general infection. But after this period the two curves widely diverge, the lower curve indicating the number of cases of latent infections of low potential at the later period of childhood, cases where the focus of tuberculosis is on the way to becoming obsolete and extinct. The upper curve, however, may represent cases where the focus of tuberculosis is quiescent but not obsolete. It is these cases of latent quiescent tuberculosis in the later years of childhood which may be the forerunners of adolescent and adult phthisis.

Von Behring[10] in 1903 endeavoured to link up tuberculosis in the child with adult tuberculosis, explaining the latter as a late fruit of seed that had been sown in infancy, and regarding the intestinal mucous membrane as the port of entrance, and tubercle bacilli in milk as the source of infection. In all the criticism that has been poured upon this theory, there has been neglect of one point which Behring emphasized—that phthisis is a disease occurring in a person already infected with latent tuberculosis, and that the peculiar character of phthisis is due to this previous infection, which without good grounds he dated back to the earliest period of childhood.

But Hamburger and von Pirquet have now established the fact of latent tuberculosis in childhood, and have traced its progress and increasing frequency through all the periods of childhood. Hamburger[11] therefore strongly supports Behring's conception of phthisis as a late sequel of a process begun in childhood. He compares tuberculosis with syphilis, and believes that primary, secondary, and tertiary stages can be marked in the one as in the other. In tuberculosis the primary stage is the first inoculation at some period of childhood—the penetration of a mucous membrane, the settlement in a gland. The secondary stage is represented by the terribly acute manifestations of tuberculous disease in childhood, abdominal tuberculosis, acute phthisis, tuberculous meningitis, general tuberculosis. But this secondary stage may never appear, and yet the tertiary phase may develop many years after in the chronic pulmonary tuberculosis of the grown man. That, so far, is an elaboration of von Behring's view. In childhood the seed is sown as latent tuberculosis; in manhood, the harvest is reaped as phthisis.

Hamburger brings *post-mortem* evidence in support of this view. In Vienna, of 175 children dying of tuberculosis, tuberculous disease of the bronchial glands was found in 97 per cent. The primary focus could in most cases be made out in the lung itself. This disease of the bronchial glands is, according to him, the starting-point of a tuberculous invasion of the lung, such invasion being delayed till adolescence or adult life.

But this pulmonary invasion may not be delayed. It may occur in childhood, producing an acute phthisis, which is rapidly fatal.

Sluka in Vienna[12] has published a series of cases of this kind, in which by the use of x rays he demonstrated in life enlargement of the bronchial glands, and later a spread of disease into the lungs. In adult life, however, it is a slow infiltration of the lung, in the first instance by way of the lymphatic tracts, and later gives the familiar picture of a chronic phthisis.

Shennan[13] has made a valuable analysis of *post-mortem* examinations in the Sick Children's Hospital, Edinburgh, and his table of glandular distribution is exactly comparable with a table given by Hamburger. In both tables the children died of tuberculous disease; the cases are thus not of latent tuberculosis.

This comparison brings Edinburgh into line with Vienna with regard to *post-mortem* material, showing for both places the great frequency of bronchial gland tuberculosis, but emphasizing also the peculiar position of Edinburgh with regard to abdominal tuberculosis.

The evidence in favour of a connexion between tuberculosis in the child and in the man thus rests on several sets of facts: On the great frequency of latent tuberculosis

Table showing Glandular Distribution of Tuberculosis.

Glands Affected.	Edinburgh (Shennan). Total Cases, 243.	Vienna (v. Ghon). Total Cases, 175.
Mediastinal alone	93 (or 39 %)	113 (or 67 %)
Mediastinal and mesenteric together	101 (or 42 %)	169 (or 100 %)
Mediastinal total	194 (or 81 %)	169 (or 100 %)
Mesenteric alone	45 (or 19 %)	—
Mesenteric total	150 (or 63 %)	56 (or 33 %)

in childhood, especially in the later periods; on the localization of this latent disease in a great majority of cases in the bronchial glands that trap the lymphatic currents through the lungs, and on the special age incidence of phthisis at the periods of life nearest to childhood.

Whether this invasion of the lungs is entirely an auto-infection from the disease in the bronchial gland, or is partly also assisted by a fresh infection, there are no means of judging. The point, in any case, is not important. The fundamental fact, both for Vienna and for Edinburgh, is that among the poor children latent tuberculosis is extremely common, and in the majority of cases is located in the glands at the roots of the lungs.

A question here arises whether these bovine infections, that have been shown to be such a special and important feature of child tuberculosis in Edinburgh, can end in adult or adolescent phthisis. The British Royal Commission only isolated 2 bovine strains from 42 cases of phthisis. The answer to the question thus depends on whether metamorphosis of bovine into human types of bacilli occurs in human tissues; and metamorphosis, though the balance of experimental evidence is against it, has not received a final scientific negative. But even if bovine infections are practically confined to child tuberculosis, there is probably a sufficient number of human infections in childhood to support the hypothesis of adult phthisis as a sequel to a focus of pre-existing local tuberculosis.

If adult phthisis be in most cases a sequel of latent tuberculosis acquired during childhood—and certain facts, both clinical and pathological, seem to support it—any administrative measures that might reduce or prevent the infection of children would have decisive effect in the campaign against tuberculosis.

I wish to express my indebtedness to Dr. Melville Dunlop, Dr. John Thomson, and Dr. J. S. Fowler, who kindly allowed me perform the cutaneous tests on patients in their wards; and also to the Carnegie Trustees for a grant which has defrayed the expenses of the investigation.

References.

[1] Hamburger, British Medical Journal, 1910, vol. ii, p. 76. [2] Von Pirquet, *Verhandl. d. Gesellsch. f. Kinderheilk.*, 1907, S. 32. [3] *Tuberculosis in Infancy and Childhood*, edited by T. N. Kelynack, 1908, p. 115. [4] Findel, *Zeitchr. f. Hyg.*, Bd. lvii, S. 101. [5] Cobbett, *Journ. of Path. and Bact.*, xiv, p. 563. [6] Fin, Report of British Royal Commission on Tuberculosis. [7] Mills, British Medical Journal, May 14th, 1910. [8] McNeil, British Medical Journal, November 6th, 1909. [9] Hamburger and Monte, *Muench. med. Woch.*, 1909, N. 9. [10] Von Behring, *Deutsch. med. Woch.*, 1903, S. 689. [11] Hamburger, *Allgemeine Path. und. Diagnost. d. Kindertuberculose*, Leipzig u. Wien, 1910, S. 98, et seq. [12] Sluka, *Wien. klin. Woch.*, February 15th, 1912. [13] Shennan, *Lancet*, January 30th, 1909.

THE *Boston Medical and Surgical Journal* quotes a report from Albany as stating that on August 26th Governor Dix announced the appointment of Dr. Charles H. Andrews of Buffalo, Dr. Lemon Thompson of Glens Falls, and Dr. Charles C. Duryea of Schenectady, as members of the board, established last year by the Bush Bill, to examine the feeble-minded, criminals, and other defectives, with a view to determining upon the legitimacy of their surgical sterilization. "The examiners are empowered to examine into the mental and physical condition, the record and family history of the feeble-minded, epileptic, criminal and other defective inmates in the State hospitals for the insane, State prisons, reformatories, and penal institutions, and one of its members is authorized 'to perform such operation for the prevention of procreation as shall be decided by the board to be most effective.' Only criminals convicted of such offences against the criminal law as convince the board that they are subject to 'confirmed criminal tendencies' come within the operation of the law."

THE SUBJECTIVE METHOD FOR ESTIMATING BLOOD PRESSURE.*

BY H. G. ARMSTRONG, M.R.C.S.
MEDICAL OFFICER, WELLINGTON COLLEGE.

IN all observations for the clinical estimation of blood pressure it is generally agreed that the most accurate method is to employ a hollow pad to encircle the whole of some portion of a limb, to attach an air-pump to this, by means of which the pressure on the limb can be increased, and a manometer to measure the amount of pressure exercised. It is also generally agreed that the only forms of manometers which can be depended on for accuracy are those which are provided with a column of mercury. The others are not to be trusted unless frequently standardized by comparison with a mercurial instrument. For convenience the arm is the limb on which observations are generally made, and the best results are obtained when the compression armlet, measuring not less than 12 cm., is applied to the upper arm just above the elbow.

The pressure in the armlet having been raised sufficiently to obstruct completely the passage of blood through the brachial artery, this, by means of a control valve, can be gradually released, and the return of the blood into the arteries below the armlet can be determined in various ways. Those in common use are (1) the digital; (2) the auditory; (3) the visual.

1. *The Digital Method.*

By keeping the finger on the radial pulse the moment of the return of blood into the vessel can be observed with more or less accuracy. This indicates the moment when the power exercised by the heart is sufficient to overcome the pressure of the armlet, and is generally accepted as the systolic pressure. The digital method does not tell anything more, and is open to the objection that it is much influenced by the personal equation of the observer. This applies not only in comparing his observations with those of others, but also to his own, made on different occasions and under different conditions. I have little doubt, from experiments made on myself, that the acuity of the tactile sense is very much influenced by conditions of health and environment.

2. *The Auditory Method.*

This was first introduced by Korotkow of St. Petersburg in 1905, and made known in England by Dr. Oliver five years later. Instead of using the finger on the radial pulse, a stethoscope is applied to the brachial artery at the bend of the elbow; on the return of the blood beneath the armlet a distinct throb becomes audible, which gradually increases as the pressure lessens, and then gradually dies away and disappears on the complete relaxation of the constricting band. The auditory method gives not only the high point—the systolic pressure very definitely—but it also gives the low point; under normal conditions the point of maximum throb is midway between these points, and this is generally accepted to be the indication of diastolic pressure. It therefore gives three points. The finding of these is of the greatest importance, as I believe that alteration of the ratio between the systolic and diastolic points is the determining factor in the elucidation of those diseases which depend rather on disturbance of physiological conditions than on mechanical obstruction. Auscultation, then, has the advantage of giving more information, more accurately, and much less influenced by the personal equation. It has this disadvantage, that anatomical variations of the parts about the elbow may cause a throb to be heard even when there is no pressure at all. I have had one case, and Sir Lauder Brunton tells me he has had another, in which the low point was zero, which, of course, vitiates any calculation of the mean pressure.

3. *The Visual Method.*

Apart from the recording instruments, such as those designed by Erlanger and G. A. Gibson, which, though accurate, are unsuitable for clinical work, the determination of the blood's return by means of the sight depends on the agitation conveyed from the armlet to a metallic needle or a column of spirit or air. The maximal oscillation of the needle or column indicates diastolic pressure. The value of the observation depends very largely on the accuracy of the instrument and the amplitude of the oscillations. For demonstration purposes the visual method is most convenient, and Pachon's instrument, which I am using this afternoon, is perhaps the best. This instrument is supplied by the makers with too narrow an armlet designed for application to the forearm. The results from this are misleading; but if a Martin's 12 cm. armlet be used on the upper arm, substantially accurate results are obtained, coinciding with those by other methods, except that the needle is more sensitive than either the finger or the ear, and the record is 2 cm. too high for the high point and the same amount too low for the low point. In comparing methods a deduction must be made for this.

4. *The Subjective Method.*

Since my attention was called to the auscultatory method, I have always employed it, finding it more accurate and comprehensive than the digital. I found, however, in taking my own pressure that I was able, with equal accuracy, to estimate by my own sensations the moment when blood commences to return beneath the armlet, and that at which, the pressure being entirely relaxed, there is no obstruction to the circulation. The throb, which is audible through the stethoscope to the ear of the observer, is equally felt by the observed. With a little education the point of maximum throb can also be determined. This, which I call the *subjective method*, is, when attention is called to it, so obvious that I would not have ventured to bring it forward had I not been requested to make it public by Dr. George Oliver, to whom I communicated the observation at the beginning of last year. He introduced a note upon it into his paper in the *Quarterly Journal of Experimental Physiology* (March, 1911). He has since written to me saying that he has constantly made use of the method, and found it accurate and of much value. In estimating blood pressure I invariably make use of the auscultatory and subjective methods together, the one to correct the other. The subjective method has this advantage, that it is free from the small percentage of error due to anatomical variation, and this disadvantage, that with nervous and excitable people it is of no use at all.

* Paper read before the Oxford and Reading Branch of the British Medical Association, on July 13th, 1912.

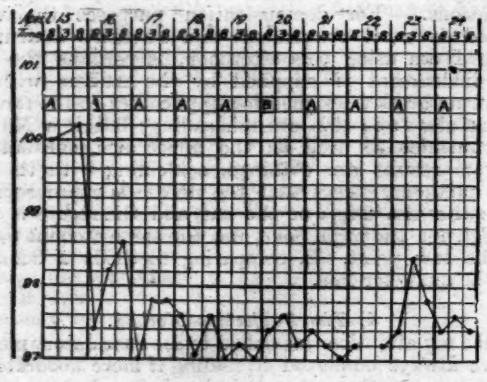
WHOOPING-COUGH TREATED BY INTRAVENOUS INJECTIONS OF IODOFORM.

BY
THOMAS W. DEWAR, M.D., C.M., F.R.C.P.E.,
DUNBLANE.

FOR many years I have been anxious to try the effect of intravenous injections of iodoform in whooping-cough, but as one generally meets with the disease in young children with very small veins who lack the fortitude to submit to the operation, I have not been able to attempt it. Apart from its utility in pulmonary tubercle, it has proved of service with me in pneumonia and many obstinate cases of pulmonary catarrh which have resisted applications of iodine, blistering, and a great variety of cough mixtures. I was glad, therefore, of the opportunity to apply it in a lad of 15, just returned from a public school where the disease was prevalent. He had the usual malaise, the suffused face and eyes on coughing; his temperature was 100°, pulse 100, and respirations 26. The whoop had developed. He could give me no idea of the number of coughs in the day, further than that they were frequent and his nights were very broken by coughing. There was very little expectoration, but on auscultating the chest catarrhal sounds were distinctly audible over both lungs from apex to base. There were no other physical signs of lung mischief. The temperature (taken orally three times a day) was normal on the third day. The whoop disappeared from the cough on the fourth day. He slept all night without a cough on the fifth day. Pulse and respirations became normal, and the number of coughs fell from twenty (urgent and prolonged) to four (simple and short) in the day, on the tenth day.

On the first two days the coughing immediately after the injection was pretty severe, but every day thereafter

it diminished till, on the tenth day, no cough followed the injection.



A, Injection of CHl3, gr. j. B, Injection of CHl3, gr. jss.

Pari passu with these changes there was a progressive sense of betterment and return to health. He felt less exhausted after the cough and his appetite improved. As I have said, the application of this treatment, even by the most dextrous, is quite impossible in children. Their veins, as a rule, are too small, even for the finest needles made, and are frequently buried in fat. Besides, very few young children could be coaxed into submitting to the daily prick of a needle, after having a handkerchief tourniquet applied to the arm, or hold the arm steady enough even if they would.

During the past nine years, since I first published this method for treating pulmonary tubercle, I have heard indirectly of alarming symptoms being attributed to the intravenous injection of iodoform. It is rather extraordinary that I should have practised it without intermission for nearly twelve years, and in that time given thousand of injections and never once met with anything worse than a local thrombosis in my earlier experience, which was due to my own want of skill. As iodoform is a power for good in such a complex disease as pulmonary tubercle,[1] and is apparently potent in pneumonia, persistent pulmonary catarrh, and whooping-cough, it is desirable that the minds of the profession should be disabused on this point. Even in advanced pulmonary tuberculosis patients can stand the continued exhibition of iodoform intravenously without any outward effect. I have shown what it can do in an adolescent with whooping-cough, and I have no hesitation in saying that an adult or elderly person who contracts the disease will suffer infinitely less from the daily dose of iodoform for ten days or a fortnight (should it be necessary so long) than their hearts and blood vessels will suffer from the strain of repeated prolonged coughing.

In this case the veins were large and well filled, and as I knew the course of treatment if successful would be short, I used 1 grain of iodoform dissolved in 10 minims of methylated ether, specific gravity 0.720, alone. In a paper in the BRITISH MEDICAL JOURNAL for 1905 I advised the addition of as much as 40 per cent. of liquid paraffin in the treatment of pulmonary tubercle, but I now think the less one can work with the better. There is no doubt that the large percentage obviates all risk of iodoform blocking in the needle or jamming the piston of the syringe, and it also greatly diminishes the irritating effect of the ether on the lining membrane of the veins. Where the veins are small, few in number, or badly filled with blood, it must be used in proportions varying from 2½ to 20 per cent., but it undoubtedly aggravates the coughing at the time, and is more apt to cause breathlessness afterwards than iodoform in ether alone, or with only a very small percentage of paraffin. In all the years I have used iodoform intravenously I have seldom met with any symptoms suggestive of idiosyncrasy, and certainly none pointing to anaphylaxis. Dr. Emery, in his paper on anaphylaxis at the Annual Meeting of the British Medical Association, 1911, alluded to this, not I think as a personal observation, but as having been recorded by Brucks and Klausner.[2] It is alleged that the shock which sometimes accompanies acute attacks of "serum disease" can be entirely controlled by the administration of ether as a general anaesthetic. Assuming Brucks and Klausner to be right, is it permissible to believe that the ether in the iodoform solution prevents this? I would also add that I have never met with symptoms of iodoform poisoning.

My object in publishing notes of this case of whooping-cough is not so much to present what looks like a specific treatment (for, as I have said, its application is circumscribed to adolescents and adults) as to direct attention to the general applicability of iodoform administered intravenously in a variety of pulmonary infections occasioned by different specific infections. Nor is its beneficial effect confined to the lungs alone. In a letter I received last month from Dr. W. Johnson Calder, of Rayton, Transvaal, he says: "I am convinced that not only is the injection of use in phthisis, but in pneumonia, asthma, and leprosy, as well as syphilis. This has been my observation, and you should note it."

In these days of vaccine and serum therapy, when a specific infection, we are led to believe by some, can only be controlled by a particular vaccine or serum, is it not possible to think that chemiotherapy may after all have been thrown over too soon? In the field of established and active disease no inconsiderable successes may yet await it.

REFERENCES.
[1] Dr. Bain, *Glasgow Medical Journal*, 1909; Dr. Crofton, BRITISH MEDICAL JOURNAL, 1912. [2] *Berl. klin. Woch.*, 1910; *Munchener*, 1910.

THE CONTAGIOUSNESS OF LEPROSY.

BY

J. W. LINDSAY, M.B.,

BELÉN, PARAGUAY, SOUTH AMERICA.

As the perennial discussion of the contagiousness of leprosy appears to have started again, as seen from the correspondence in your issues of March 23rd and 30th and April 20th, I have thought it might be interesting if I gave the history of various leper families resident in this Republic of Paraguay.

I daresay medical men resident in leper countries are as a rule so thoroughly convinced of the contagiousness of leprosy that they do not think it worth while to write to contradict statements made to the contrary. After an experience of twelve years in a leper country I am able, from personal observation, to support the view of your correspondent of April 20th, to the effect that "leprosy is not hereditary," and that "leprosy is contagious." Whether it is spread (as he says) wholly and solely by contagion, immediate or mediate, or whether cases may arise *de novo* from eating fish or other foods badly cured, is a difficult matter to decide. My own idea of the question will be gathered from what follows.

It is almost certain that for the rapid spread of leprosy in a country certain favourable conditions are necessary. It may be, as the advocates of the fish theory hold, that the conditions necessary are those of diet—that leprosy has been most prevalent when a certain kind of diet has been in general use.

At the present time in Paraguay leprosy is spreading like wildfire. During the last ten years the number of lepers has doubled—perhaps even trebled. In that time there has been no change in the mode of living of the people as regards their diet.

At the present time leprosy is far more contagious in Paraguay than tuberculous disease of the lungs. I could give various instances of phthisical patients who have lived in the midst of their families in their little windowless huts, have expectorated over floor and walls or wherever convenient, and yet their families, husbands or wives or children, have not contracted the disease as far as can be seen. A man in a house not a square from here is in the last stages of phthisis. His wife and two children have slept in the same room and eaten with the patient now for six years. Not one of the family shows a sign of phthisis. Signs may appear later; but what I wish to show is that tuberculosis has not found a suitable home in this country such as it has in Great Britain. Far otherwise is it with leprosy.

Ten years ago I first saw A. M. He had a black patch covering the place where his nose had been. He was in the last stage of leprosy. Two years later I was called to attend his son, who had been stabbed in the chest. The leper father was squatting in a corner of the hut. A year later he died.

Two years later his daughter consulted me. I diagnosed leprosy. She is now disfigured with the nodular form of the disease. Two years ago a son of the old leper, brother of the last, consulted me for indefinite symptoms. I suspected leprosy. Now the patient's face has the typical roughness and duskiness of the nodular disease. These two patients had always slept in the same room as their father, eaten from the same dish, and used the same bombilla, or metal tube for sucking maté.

R. R. I had known as a leper for many years. Three years ago I saved his face from being eaten away by maggots by destroying the latter with powdered calomel. Last year R. R. died of pneumonia. In the same room lived his wife and grown-up family. To-day his daughter, aged 25, came to consult me. Three months ago I had seen her and had noticed nothing in her appearance. To-day the expression of her face made me look twice. It was roughened with nodules and discoloured. She showed me her shoulders, and they presented a typical leprous eruption. She had come to consult me about another trouble, and asked me, by the way, if I could give her some medicine for her brother, who was worrying about a skin disease that had appeared on his shoulders, "the same as this on mine," she added, as she bared her own shoulder. The son and daughter had lived in close proximity to their father all their lives, and have contracted his disease.

A European, C. D., had been known as a leper for many years. Twelve years ago I became acquainted with him. He was then a typical leper with leonine face. He went on a visit to Europe. In H. he consulted a doctor about his eyes. The doctor diagnosed his leprosy and seemed scared. His remarks or behaviour scared C. D., for he left H. the same night and embarked again for South America on the first steamer. In Buenos Aires he consulted a doctor about his eyes. The doctor got a shock, which he unconsciously communicated to C. D., who took the first steamer up river to Paraguay. Here he had the pleasure of reading in a European paper of a mysterious case of leprosy that had been seen in H. in such an advanced stage that the medical man had reported it to the authorities. When the authorities came to look for C. D. he had disappeared. He was a very nice old man, and I had him brought out to our village and placed in a small isolated house. He lived there for about nine months and then died. The nearest native house was about fifteen yards from where C. D. lived and died. The native was M. Q., and he used to sit with C. D., and even share his meals. The day that C. D. died, and before we could get him buried, M., wishing to have some remembrancer of his old friend, went rummaging in the latter's trunk and appropriated his watch and various other things! Two years later M. consulted me about a scaliness of his ears. He is now a confirmed leper. For three years he lived in the same house as his young family; then he went to live in a hut in his plantation. Within the last three years three of his daughters, aged 20, 15, and 12 have become lepers. Within the last two years the children of a neighbour have become infected, and in one house twenty yards from M's. there are three leper children.

Six years ago I had occasion to visit an old man and his wife in connexion with a matter of business. Medical certificates of their state of health were required in a legal case. I found that they were both lepers. Two years previously a daughter of theirs had consulted me for heart trouble. She suffered from breathlessness and palpitation. Her pulse was generally between 120 and 140. I could find no cause for the rapid heart action. I tried various remedies, but after some months she left off taking medicines as they did her no good. When I saw her father and mother were lepers I immediately asked for her. Her parents laughed and said that she was hiding in the house as she had grown so ugly! I called her out; her face was horribly disfigured with tubercular leprosy. I came to the conclusion that the tachycardia that had puzzled me years before had been due to a very severe invasion of the lepra bacillus. The case developed very rapidly. The patient had lived with her parents, eaten with them and slept with them, and had become a leper.

M. C., a woman, has been a leper for some years. She lived in the house of her sister, R. C., who had a young family. When M. C. began to get disfigured in the face she went to live in another part. R. C's. oldest boy of 13 is now a typical leper, and his younger brothers are all attacked by the disease, a fact that can be considered certain, as the anaemia of the children will yield to no treatment.

From these cases and from the histories of many others that I might quote I am absolutely convinced that in this country at any rate leprosy is spread by contagion, either immediate through contact with a leper or his discharges, or mediate through conveyance of the infection by insects. In this country also there is no doubt that, owing to the conditions being so favourable to the rapid spread of leprosy, the disease is far more infectious than is tuberculous disease of the lungs, for which the conditions are not so favourable. It may be also that the present invasion of leprosy is due to a a more than usually virulent strain of the bacillus. It has nothing at all to do with fish-eating, as the people do not eat fish, and although they are all Roman Catholics, the only Friday when they do not eat meat is Good Friday!

Your correspondent of April 20th says: "As I have lived and worked in a very large leper asylum for nearly seven years, and *am in consequence myself a leper*," as if to imply that such a fate was inevitable—and I quite agree with him. In this country, with the virulent type of leprosy at present so rapidly spreading, it would be almost impossible for a medical man to do his duty to his leprous patients in a leper asylum (if such existed) and escape infection.

Your correspondent mentions a curious point: "With regard to myself, I am not at present contagious."! How does he know? Perhaps the most contagious stage of leprosy is before any outward signs are visible, and when children and relatives and the patient himself have no idea of danger. When outward visible signs appear I fancy it may then be too late to think of separating from one's family with the idea of saving them from the disease.

Two days ago a typical tubercular leper came to my dispensary. His face and his hands were greatly disfigured. Four years ago he had gone to a general hospital in the capital, Asuncion, to consult about his nose. Years before he had had ulcers in his legs. The doctors in the hospital diagnosed his case as syphilis and treated him with injections of various kinds for four months. He then had a peculiar dryness in his nose, which on occasion would also get stuffed up. It probably had been the first lesion of his leprosy. For three years he lived with his family and worked with other men with the scarcely noticeable leprous lesion in his nose, and only six months ago did any other symptom appear, and then his hands and face began to swell, and to-day the diagnosis is unmistakable.

The diagnosis at the beginning, and perhaps for many years, is extremely difficult, in many cases, but in leper countries medical men very often can diagnose with certainty even incipient cases. The sanitary board in the capital has very strict regulations regarding lepers, their isolation, their non-admission to public buildings and public conveyances, etc., but the regulations are a dead letter, and nobody seems to mind where the lepers go or what they do. Up to the present it has been impossible to enforce the law for their isolation. When compulsory isolation becomes possible in this country, then we may hope that the disease will be checked and in time stamped out.

STRANGULATED INGUINAL HERNIA IN AN INFANT.

By G. E. E. NICHOLLS, M.B., CH.B.VICT.,
CROYDON.

THOUGH there is no point of an unusual nature to be related in connexion with this case, yet as strangulation at so early an age as eight weeks is by no means common the report may not be without interest to many.

A male child, born on March 16th, 1912, had been circumcised on April 4th owing to marked phimosis.

On the evening of May 12th, the child being then eight weeks old, I was called in as it had been crying almost continuously since mid-day, he was also said to have vomited twice during the afternoon; this was not altogether remarkable as there had been considerable trouble in feeding for the past two or three weeks. On examination, however, the patient was found to be in a somewhat collapsed state, with very rapid and feeble pulse, and with a pinched and anxious expression. A small firm mass was found on the right side of the scrotum, obviously produced by a right inguinal hernia.

A careful attempt at reduction under an anaesthetic proving useless, I decided to operate at once. The hernia consisted of a knuckle of small intestine; it was oedematous and of a dark purple colour, but the serous coat was not without lustre, and it might evidently be replaced with safety. The sac contained about a drachm and a half of pale clear fluid. The constriction was at the internal ring, and there was some difficulty in returning the gut, the ring having to be incised in one or two places. The sac was of the complete funicular (so-called "congenital") type, the testis being found within it at the lower end. The sac was ligated high up, and two buried sutures inserted to diminish the size of the external ring. Recovery was complete and uneventful.

This was apparently the first occasion on which the hernia had come down, and the antecedent gastric disturbance, involving repeated vomiting and consequent raised intra-abdominal pressure, may no doubt have been a factor in the production of the hernia. There was no suppression or retention of urine, as has often been noted in these cases of strangulation in infancy.

EIGHTIETH ANNUAL MEETING

OF THE

British Medical Association.

Held in Liverpool on July 19th, 20th, 22nd, 23rd, 24th, 25th, and 26th.

PROCEEDINGS OF SECTIONS.

SECTION OF PHARMACOLOGY AND THERAPEUTICS.

Professor W. E. DIXON, F.R.S., President.

PRESIDENT'S INTRODUCTORY REMARKS.

PROFESSOR DIXON, after extending a hearty welcome to those who were attending the meeting of the Section, referred to the increasing interest which was being taken in the study of pharmacology. As a sign of this he mentioned that since the Section had last met a *Journal of Pharmacology and Therapeutics* had been established in this country. The journal was published simultaneously in the United States and in Great Britain, under the joint editorship of Professor A. R. Cushny and Professor Abel. He regarded the institution of a journal wholly devoted to pharmacology and therapeutics as an important step forward.

DISCUSSION ON THE TREATMENT OF NON-VALVULAR CARDIAC DISEASE.

OPENING PAPERS.

I.—G. A. GIBSON, M.D., LL.D.,

Physician to and Lecturer on Medicine in the Royal Infirmary, Edinburgh.

THE terms defining this discussion exclude every affection of the valves, and it appears to me that it will be more useful for us also to pass by all diseases of the pericardium. My remarks, therefore, will be simply confined to disorders of the myocardium.

The cardiac muscle is not only affected by microbic infections, but it is also subject to the influences of chemical poisons, which may be either of external or of internal origin. Increased and diminished glandular secretion may also bring about changes in the functions or the structure of the myocardium. The effects of strain, whether physical or mental, must not be left out of account. In elderly people many senile alterations take place which may be distant results of some of these other changes already referred to, or may simply take their origin in deficient metabolic processes. It is not necessary, in this connexion, to make any reference to the undoubted fact that the myocardium undergoes profound alterations as the result of pericardial and endocardial diseases such as we are precluded from referring to.

Such is a very short sketch of the pathogenetic factors leading to the alterations in the myocardium.

The effects brought about by myocardial diseases may be recognized by general symptoms—cyanosis, dyspnoea, angina, oedema, asthenia, insomnia, and many other evidences of disturbances of the circulation. Changes in the arteries are frequent accompaniments of myocardial affections; alterations in the walls and modifications of the pressure are often discovered. Disorders of the rhythm and the rate of the pulsation are never to be minimized in importance, as these are commonly present, and may be made out both by the ordinary clinical methods and by our modern modes of investigation. In this connexion you will allow me, once more, to affirm my belief in the results obtained by the sphygmomanometer as well as our modern instruments for recording changes in the movements of the heart and the arteries. The size of the heart must always be ascertained, not only by percussion, but, when possible, by orthodiagrams and skiagrams. The presence of murmurs may be most helpful, inasmuch as myocardial changes may be present without valvular diseases, murmurs of escape in such cases being produced by lack of tone in the muscular tissue surrounding the orifices. It seems to me that it will not be long before we are able to estimate the force which is generated in the heart by the methods summed up under the term "ergography," while a measurement of the electromotive force of the heart is now almost within the realm of practical medicine. We are able to measure the resistance of the body, and we are able, approximately, to estimate the differences in electromotive force, by means of the string galvanometer. Many years ago the subject began to occupy my attention; but as the only available instrument at that time was Lippmann's capillary electrometer, you can easily understand that the difficulties were insuperable. In all these methods it is our duty to ascertain, in so far as it lies within our power, the condition of the circulation from the examination of the heart and the arteries. One other point may be referred to—it is one which Oliver has, with his usual acumen, made practicable—an estimation of the return of the blood to the capillaries after it has been expelled by pressure.

Having thus briefly sketched the various conditions and their symptoms, along with the methods of investigation, let me turn to the management of myocardial changes. You will probably be tired of the trite statement that rest is the best of all cardiac tonics. At the risk of being tedious, let me once more reiterate the axiom. It must, however, be real rest, accompanied by abundance of fresh air and associated with a sufficiency of sleep. During the period of absolute rest, gentle massage and passive exercises are most helpful, by aiding metabolism without fatigue. The mention of massage leads me to refer to the employment of baths. In minor degrees of cardiac weakness, baths, especially the effervescing sodium and calcium baths, are undoubtedly useful.

As regards exercises, as has been stated by me on several previous occasions, the various modifications of Ling's Swedish movements are very much more satisfactory than the resisted movements which have been so much vaunted during recent years.

The diet must be carefully studied, the chief principle involved being the separation of solids and liquids, while thorough elimination must be attended to. In acute infective cases the treatment of the cause, if still present, must be carried on. As mentioned in the Address in Medicine yesterday, changes in the suprarenal bodies are apt to bring about fatal fall of arterial pressure, which must be combated by the use of suprarenal and pituitary extracts. In the chronic results of infective causes the use of iodides, combined with digitalis or strophanthus, cannot be overestimated. For those cases in which there is tachycardia or palpitation the bromides, along with digitalis, will be found most useful; the particular bromide which commends itself to me is calcium bromide. Will you allow me to mention that, as calcium is a bivalent element, the dose of calcium bromide requires to be proportionately less than of the bromides of the halogens?

In bradycardia, belladonna or atropine, with or without strychnine, has been of most use in my own experience, but in certain cases digitalis or strophanthus must be employed in order to keep up the tonicity and contractility of the ventricles.

For chemical poisons, such as tobacco and alcohol, total abstinence from these substances must be enjoined, and, along with general treatment, strychnine, in combination with hydrobromic acid, seems to me the best remedy.

When we are face to face with those conditions which we are fain to term "gouty," for want of a better term, the use of colchicum and the alkalis still give us the best results. In my own opinion, the reason why colchicum is so often despised is that we do not use it in sufficient doses.

Turning to the glandular conditions, when there is any myxoedematous tendency, such as is so common about the climacteric period, the use of thyroid extract gets rid of all the symptoms of cardiac dilatation and the patient enters upon a life of comfort.

In exophthalmic goitre no remedy approaches, even distantly, suprarenal extract in importance, and it may be regarded as almost a specific for the affection. The use of ovarian extract and of didymin in middle-aged people is

often extremely helpful when there are symptoms that suggest their employment.

In all cases of arterial changes with high pressure the nitrites and the iodides are of paramount importance. It need hardly be added that in such cases the use of abundant fluid to increase tissue changes will be found, along with free elimination, of the greatest value.

Perhaps you will permit me, finally, to refer to the most interesting work of our President on extracts obtained from the heart in varying conditions of activity. Some years ago, at a meeting of the Physiological Society in Edinburgh, the pleasure was afforded me of discussing with Professor Dixon the remarkable results which he had obtained from the use of such extracts, and it appears to me that this line of inquiry will be of the greatest utility. Our President informs me that he has recommenced his investigations along those lines, and we may look forward with confidence to much real help from those researches.

II.—Professor A. R. CUSHNY, F.R.S.,
Professor of Pharmacology, University College, London.

THE subject of discussion is so wide that it is impossible to treat of it as a whole in the opening remarks, and I must therefore ask you to allow me to devote my time to a small part of the treatment of heart muscle affections to which I have given my attention—that, namely, which involves the use of drugs of the digitalis group. These drugs have been so often the subject of experimental investigation that the broad characters of their action may be said to have been definitely determined, and a few years ago I pointed out that in their case, as in that of many other substances, the chief present need was more accurate and detailed clinical observation. An opportunity of following such observations was offered me by Dr. James Mackenzie, and I propose to lay before you to-day a short statement of the results obtained by Mackenzie, his pupils and myself from the use of these remedies.

First, as to the drugs used, we have tested a number of different preparations, and have found none superior to the tincture of digitalis for ordinary treatment. This is not always of uniform strength, it is true, but that supplied us by good firms has proved wonderfully constant, whether its toxicity was tested on frogs or its therapeutic effect was estimated by the results on patients. It may be questioned whether the so-called pure principles of digitalis, obtainable commercially, are more constant in their composition and action, and there seemed no object in undertaking the very laborious work of making a detailed comparison of their effects with those of the pharmaceutical preparation. Strophanthus and squills are still supposed to differ somewhat from digitalis in their effects, and one of our first series of observations was directed to determining whether any such difference could be made out. It may be stated at once that the three drugs showed no essential variation in their action on the heart. The tinctures of strophanthus and squills were weaker in their action on patients than that of digitalis, nearly twice as much being required to elicit changes in the heart, but these changes were the same in character. In some cases strophanthus and squills seemed to have less tendency to cause gastric disturbance than digitalis, but this was not constant, and, on the other hand, diarrhœa was rather oftener seen under them. On the whole, the impression was obtained that digitalis tincture is more certain in its effects on the heart than either strophanthus or squills, but some patients seem to tolerate the last two more easily than digitalis, so that these may be regarded as substitutes for the chief drug only. One point must be borne in mind in comparing these three—namely, that their active principles readily undergo decomposition when the tincture is diluted with water, and this is especially marked in the case of strophanthus. The tinctures should be dispensed as such or diluted only with alcoholic preparations, with directions to the patients to take them in water, if necessary.

Mackenzie has pointed out that the response to digitalis differs to an extraordinary extent, according to the condition of the heart and the character of the rhythm. Taking first those patients who offered definite evidence of cardiac insufficiency in breathlessness and pain on exertion, but whose heart rhythm originated in the normal pace-maker, digitalis had comparatively little effect on the pulse-rate, and while the improvement occurred during treatment, it was comparatively slow, and did not show the striking features of the second class. In fact, it was sometimes difficult to determine how far the improvement arose from the rest in bed and other treatment, and how far it could be ascribed to the drug. In general, the improvement in these cases was accompanied by no definite decrease in the pulse-rate, but in some the pulse showed irregularities in the form of auriculo-ventricular block or of very marked slowing of both auricle and ventricle. This irregularity and slowing disappeared in most cases under $\frac{1}{50}$ grain of atropine, showing that they arose from inhibitory action; but in two cases this proved not to be the case, the irregularities persisting under quantities of atropine which were sufficient to eliminate all inhibitory action. In general, it may be said that in these cases of myocardial disease, in which the rhythm of the heart originates in the normal position, the beneficial action of digitalis may be very slight or entirely absent. When slowing occurs, it is generally inhibitory in character, though in exceptional instances it may arise from direct action on the muscle.

The most striking results of digitalis are seen in cases in which the pace-maker has lost its leadership and the rhythm of the heart is given by the fibrillating auricle. But even in this form of cardiac disease the results are not by any means uniform. For when auricular fibrillation is not accompanied by a rapid pulse, there is again no very striking effect from digitalis, hardly more so than in the cases mentioned in which the rhythm is normal in origin. Improvement occurs, but it is slowly elicited, and there may be difficulty in determining how far it follows from the drug and how far from the rest and similar measures adopted. When, however, auricular fibrillation is accompanied by a very rapid irregular pulse, the picture presented by digitalis treatment is very different. Here its administration is followed by a rapid fall in the pulse-rate and a concomitant improvement in all the symptoms. These are the cases in which immediate relief is required to avert rapid failure of the circulation. The improvement in the general symptoms may be measured by the slowing of the pulse; as this sets in, the breathlessness, uneasiness, and pain lessen, the œdema disappears and the urine excretion rises, and the patient, if not restored to health, at any rate is able to go about and undertake such light work as is possible for one whose heart muscle is distinctly impaired. The slowing of the pulse in these cases may be used as a gauge of the action of digitalis, and also as an indication as to whether the treatment should be continued. The slowing is not, as has been supposed, inhibitory in origin, for it remains after large doses of atropine. It must therefore arise from some direct action on the muscle of the heart. The auricle continues to fibrillate as before, and the digitalis may therefore be presumed to act either in the bundle of His or in the ventricle itself. Arguments may be offered for each view, and the question must remain open until further observation determines the point.

On comparing the results of digitalis in these various forms of heart disease, one point emerges clearly, I think. The heart muscle impaired by disease is capable of responding to the drug by improvement, whether the impulse arises from the normal pace-maker or not. But when the impaired muscle is further exhausted by multitudinous demands on it from the fibrillating auricle the benefit is especially apparent. This exhaustion of the enfeebled ventricular muscle through its attempt to respond to the auricular impulses is most characteristically seen in auricular fibrillation, but is really the same in character as the exhaustion arising from great acceleration, even when the normal rhythm of the heart is maintained. In myocardial degeneration any physical exertion leads to breathlessness and heart pain owing to the exhaustion of the ventricle, and the patient soon learns to relinquish any effort which has this effect. In auricular fibrillation the same burden is laid on the ventricle by numerous impulses sent from the auricle, and the same exhaustion follows; but the patient can no longer control this exhaustion, for the demands made on the ventricle are now no longer under the control of the will, but arise from the uncontrollable auricle.

As in the one case digitalis treatment may enable the patient to undertake more active exertion than formerly

without exhaustion, so in auricular fibrillation the ventricle becomes more able to meet the demands made of it by the multitudinous impulses from the auricle. At the same time the rhythm is reduced, either because the strengthened ventricle becomes more refractory, or because fewer impulses reach it from lessened conductivity in the auriculo-ventricular bundle.

The inefficiency of the ventricle is the important underlying cause of the circulatory disturbance, though this may be exaggerated by the presence of auricular fibrillation. It is as yet impossible to define this inefficiency more closely, but it has long been known as dilatation, loss of tone, etc. It is very difficult to determine any change in the dilatation of the heart under digitalis. Careful percussion failed to show any change in the limits of cardiac dullness in many cases in which obvious improvement in the symptoms occurred under treatment. Only in one case was there an obvious change. Here no apex impulse could be made out before treatment, a diffuse movement in the epigastric region only being recorded, while after treatment a very well defined apex beat was present.

Digitalis was given in these cases in full doses to begin with, a drachm a day being prescribed. This very often led to headache, nausea, or vomiting after a few days, or the pulse showed distinct effects, and the drug was then abandoned for a few days and then resumed with rather smaller doses. The dreaded cumulative effects of digitalis proved not to be serious, though the pulse ought to be watched day by day. In many cases the best results were obtained at first only when the largest doses were taken which did not induce gastric symptoms. After a course of digitalis lasting for a fortnight or more, the patient often relapsed on giving up the drug, the same symptoms recurring as in the beginning. Renewed treatment restored the improvement, and after a more prolonged course a relapse was often much less in extent, and finally a satisfactory condition could be maintained with comparatively small quantities or with larger amounts taken at intervals of several days.

One great limitation in the use of digitalis is caused by the slowness with which its action is elicited. Rarely is any distinct change to be seen before the fourth day of treatment, and this precludes its use in the most acute cases. In order to elicit a more rapid effect we have injected strophanthin intravenously ($\frac{1}{250}$ to $\frac{1}{150}$ grain) in a number of cases with most satisfactory results. The action is in every way similar to that obtained by tincture of digitalis given by the mouth, except that the results are obtained in hours instead of days, the pulse four to eight hours after an intravenous injection of strophanthin being comparable to that after about a week of digitalis. The treatment in cases of emergency would thus seem to be to commence with an intravenous injection of strophanthin, and to continue with the daily administration of digitalis tincture by the mouth. The use of digitalis in such acute conditions as heart failure in pneumonia has long been a matter of dispute; and, indeed, the slowness of its action seems to contraindicate its use here, and at the same time to suggest the substitution of strophanthin intravenously.

The action of digitalis is of course to be aided by other remedies where possible. For example, in some cases of oedema, great benefit may follow the addition of caffeine and theobromin, but I cannot agree that these act through changes in the heart. In others, the weakness of the heart may be compensated by dilatation of the vessels by some of the nitrite series, and others still may improve under bromides and digitalis.

DISCUSSION.

Dr. JOHN HAY (Liverpool) said: The heart muscle affections apart from valvular disease, occurring in those over middle age, can be arranged with fair accuracy in four groups, and for the purposes of this discussion it is well to consider patients under the following classification: (1) Obese patients with cardiac disability; (2) patients with arterio-cardio-sclerosis with or without hypertension; (3) those patients who are afflicted by recurring extra-systoles, with no other distressing symptom; (4) patients suffering from auricular fibrillation, in the absence of valvular disease. In every case, before deciding on therapeutic measures, I think it wise to obtain certain data, such as the relation of the obesity to the general musculature of the body, the extent to which sclerotic processes are present, the blood pressure, and the intrinsic nature of any arrhythmia which is present. The general treatment in these cases is, with one or two exceptions, much the more important. Drugs take a secondary place. The value of treatment must be estimated by its effect in diminishing the distress of the symptoms in each particular instance. One almost feels it necessary to apologize for emphasizing the importance of rest, and the fact that the best place in which to rest is bed. For example, one finds that troublesome anginal pains sometimes disappear when the patient retires to bed in the early evening instead of remaining up and attempting to take his ease in a so-called easy chair. The value of chloral in soothing an irritable patient and obtaining a good night's rest, and, possibly taking the strain off the heart by slightly diminishing the arterial tension, is becoming better recognized daily. There seems to be no reasonable grounds for the dread with which many practitioners still view this valuable drug in cases of myocardial disease. The ameliorative action of the iodides and nitrites, when given to patients with arterio-sclerosis and raised blood pressure, is largely due to the fact that they also diminish the load under which the heart is staggering, and thus afford it relative rest. It is almost paradoxical that one should, in the same breath in which the value of rest is emphasized, plead for the value of exercise, and yet no more fatal mistake can be made than to neglect the importance of this factor in therapeutics, especially in the first and second groups. The determining limits as to the extent of exercise are generally obvious to the patient. Each patient must be a law unto himself; the slightest appearance of dyspnoea, palpitation, precordial pain or distress, should at once be recognized as an indication for diminution or cessation of physical effort. The paper by Dr. Cushny has dealt almost entirely with digitalis and its specific uses. He has pointed out that digitalis diminishes the excitability of the myocardium, and here one finds a possible explanation of its value in patients belonging to Group 3, in whom extra-systoles are the dominant feature. The digitalis is then usefully combined with the bromides. In all cases of auricular fibrillation with heart failure digitalis must be given a fair trial—one is most hopeful of obtaining a response when the heart's action is frequent. In every such patient the exhibition of digitalis is a therapeutic experiment. The preparation of the drug should be of known potency. The patient should be in bed and carefully supervised. The digitalis should be steadily pushed until the heart steadies down or until definite toxic results are obtained, showing that the limit of toleration is reached. The total quantity of digitalis required to produce the desired result should be noted in each case for reference later. If there is reason to believe that the vomiting is due to gastric irritation, use hypodermic medication. When the pulse is alternating in quality with other signs of cardiac failure, digitalis must be used tentatively in smaller doses. It sometimes improves the heart condition, although the outlook is extremely grave when this phenomenon has once appeared.

Dr. F. W. PRICE (London) said: I shall confine my remarks to a few points in respect of chronic myocardial disease following infection by rheumatism, and cases of degeneration of the myocardium (whether fibroid or fatty), and which are often associated with arterio-sclerosis of the coronary arteries or of the aorta. It seems to me that in regard to both groups of cases certain fundamental principles should always be remembered. Given a case of damage to the myocardium, either as a result of a former attack of rheumatism or of degenerative processes, it is of the utmost importance, first, that the fact of damage should be recognized, and, secondly, that its degree should be approximately estimated. Now, in regard to both these points there are other and more important criteria than the results of ordinary physical examination (such as the amount of enlargement and the presence of murmurs), which are not nearly sufficiently appreciated. Most important of all is the question of the efficiency of the working power of the heart. Is this impaired, and if so, in what degree? In trying to estimate these it should be remembered that each individual has his own normal standard—that is, the natural standard varies in each

individual, or, in other words, each individual possesses his own standard of the heart's strength, and each knows the amount of exertion he can indulge in without producing cardiac distress. Providing temporary and extrinsic causes (such as sleeplessness, temporary physical or mental overstrain, and anaemia) can be excluded, the fact of a departure from this standard and the degree of departure may be taken as fairly indicative of the presence and degree of myocardial damage. The commonest symptoms of cardiac distress are *undue* shortness of breath, palpitation, or fatigue, or precordial pain, on exertion. In almost all cases it is only after these have set in that objective signs, such as increased pulse-rate, dilatation, and dropsy appear. Later, these symptoms of cardiac distress are present even when the patient is at rest. It may here be mentioned by way of parenthesis that, apart from symptoms, auricular fibrillation may be the only indication of myocardial involvement. Now, given a damaged myocardium, in my view one of the most unfortunate of things is the fact that such a large proportion of patients endeavour to live beyond the limits of the heart's power—that is, as though their myocardia were normal. When this is the constant or fairly frequent habit of a patient, it necessarily throws a heavy strain upon the organ, and adds to the damage already done. It is of supreme importance that this should be avoided. The medical attendant will have done the greatest service to his patient if he succeeds in persuading him to acquire the habit of always living within the limits of the heart's power. I believe that in this respect it is better to be frank with the patient, at the same time letting him understand that he will be amply rewarded for curtailing his physical and mental activities in sufficient degree. It is impossible to give hard-and-fast rules, for the reason that the constitutions of different patients differ enormously. One patient can cycle but cannot play tennis without untoward results, while in the case of another patient the reverse holds good. But there is a cardinal principle which affords a good guide. It is this: Any exertion which the patient indulges in should not produce *undue* breathlessness, palpitation or fatigue, or precordial pain. On the other hand, any exertion short of producing these is usually not harmful. The importance of diminishing the amount of mental work is not sufficiently realized. When there are indications of progressive exhaustion of the heart's strength, the amount of effort should be proportionately reduced. Confinement to bed may be necessary. In my experience prolonged rest in the recumbent posture is of considerably greater value in cases without auricular fibillation than in cases with it. Its place in the treatment of the former group of cases is of supreme importance. If there is an unduly excitable nervous system it should be treated. It is difficult to exaggerate the importance of sleep; every effort should be put forth to secure an ample amount. In regard to the employment of the digitalis series, as in vavular disease, there is a fairly sharp line of demarcation between two groups of cases—(1) those cases with auricular fibrillation, and (2) those cases without auricular fibrillation. In regard to the former, brilliant results often attend the administration of the drugs. My custom is to give a drachm of the tincture of digitalis per day until some definite reaction is obtained (such as nausea, vomiting, headache, diarrhoea, a slow pulse, or coupling of the beats), and then to stop the drug. In most cases there is a relapse to the former condition in about a week after the drug is stopped. The indication, then, is to find out what dose will best maintain the improvement without producing any unpleasant symptoms. In regard to cases unattended by auricular fibrillation, I confess to great disappointment with the results of the administration of digitalis or one of its allies. In my experience, the proportion of cases in which substantial results follow is small, and smaller than in cases of valvular disease without auricular fibrillation. In these cases much may be done by prolonged rest and other means. If these are not efficacious, in my view the outlook is very gloomy. In a paper which I am presently to read before the Section I hope to show that there appears to be no evidence that digitalis increases the blood pressure in man by constricting the peripheral vessels, and, if that be so, the important practical point will have been discovered that there is no risk in administering the drug in cases of degeneration of the walls of the blood vessels or of supernormal blood pressure. And, lastly, in regard to the treatment of supernormal blood pressure, which is so often present in cases of myocardial degeneration, in some investigations at the bedside on this subject, I have been very disappointed with the permanent results following the administration of the so-called appressors. I think far better results are obtained from rest, the avoidance of physical and mental overstrain and worry, free elimination, and exercising care in regard to the amount of food, especially diminishing the amount of butcher's meat and strong soups.

Dr. D. W. SAMWAYS (Mentone, France) said there was some danger that the discredit in which the physical treatment of cardiac cases by exercise, baths, etc., seemed to be held might be pushed too far. It was important to retain the good while rejecting the evil, and not go always to extremes. Reasonable exercise was necessary for the body generally, on whose well-being the heart itself depended. The whole man was not a heart. Warm salt and carbonic acid baths, the speaker, when he practised at Brides and Salins-les-Bains, in Savoy, had found would almost always slow the pulse of patients, sometimes considerably and enduringly. Rest was very important in all cases, but in bad cases bleeding was occasionally the best and quickest way to introduce rest. It diminished the load on the heart, and its effect in that respect was sometimes very encouraging. It should always be remembered that the heart, if dilated, laboured under very heavy mechanical disadvantages; for an enlarged chamber required a much stronger contraction of the heart muscle than did a smaller chamber to produce the same pressure, per unit area, on its contents. It was therefore very important that any deficiency in the contracting power of heart muscle should be met by decreasing as soon as possible the size of any enlarged chamber.

Dr. GIBSON, in reply on the discussion, said it was a real comfort to him to find himself in almost complete accord with Professor Cushny in his remarks. Fearing that the addition of other substances might effect changes in the active principles, both of digitalis and strophanthus, it had been the speaker's habit for many years to keep these substances by themselves, or with some other tinctures, simply adding them to the other remedies at the time they are to be taken. It was further satisfactory to have the assurance of Dr. Cushny that the tincture was far better than any other preparation. Many of the actions, both of digitalis and of strophanthus, appeared to be brought about by their effects on the tonicity of the heart, and the recent work of Cameron, carried out in Baltimore, distinctly proved this fact. Its employment in partial heart-block, and in auricular fibrillation, although not yet thoroughly understood, would probably be found in the future to be of the highest importance. Probably many present used digitalis in too small dosage; the speaker well remembered a distinguished patient who appeared to owe his recovery, when quite an old man, to the use of tincture of digitalis in teaspoonful doses, administered by the direction of the speaker's old "chief." Strychnine undoubtedly increased the tone of the heart; this had been proved by Cameron. He was previously unaware of the existence of pressor substances in the renal secretion of acromegaly. In regard to the employment of chloral, he agreed with Dr. Hay, and endorsed his views on the dangers of alternating action of the heart. High value was certainly to be set, as urged by Dr. Campbell, on the encouragement of the patient when there was no real danger. In many cases merely assuring the patient that there was no imminent risk sufficed to remove the symptoms.

THE STANDARDIZATION OF CARDIAC TONICS.

By A. J. CLARK, M.B., M.R.C.P.,
British Medical Association Research Scholar.
(From the Pharmacological Laboratory, King's College, London.)

THE present methods for the physiological standardization of cardiac tonics are admittedly imperfect, and any suggestion for their improvement deserves careful consideration. In a communication to the Physiological Society in 1908, Dr. Waller[1] described the action of various

preparations of digitalis upon the isolated frog's sartorius, and concluded that

the method is very applicable to the physiological standardization of digitalis and allied substances.

From these results and from similar experiments with many other drugs Dr. Waller further concluded that

a general parallelism holds good between physiological activity, measured by the response of muscle, and toxic or narcotic power, measured by killing power per unit-weight of living animal.

Fig. 1.—Tracings showing the action of strophanthin upon the isolated frog's heart. (i) Heart perfused with strophanthin 1 in 100,000: A = drug introduced. (ii) Heart perfused with strophanthin 1 in 1,000,000: A = drug introduced. (Upstroke systole. Tracings read from left to right. Time in minutes.)

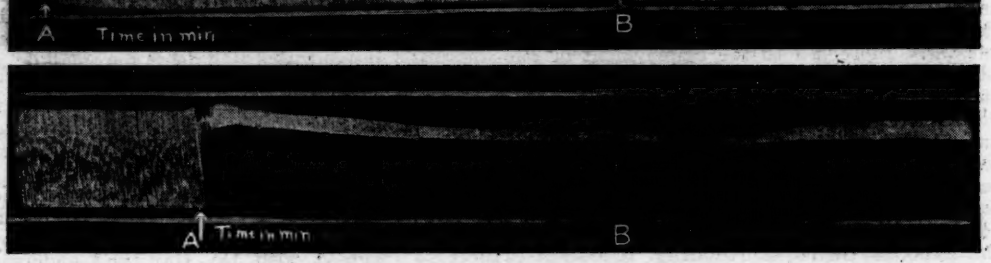
The experiments described below were undertaken to test these statements in the case of two drugs of the digitalis series—namely, strophanthin and digitonin; the action of these two drugs was determined upon the intact frog, upon the isolated frog's heart, and upon the isolated frog's sartorius.

Method.

The drugs used were amorphous strophanthin Kombé (Wellcome brand, Burroughs and Wellcome) and digitonin (digitalinum cryst. or digitonin cryst., Merck).

In estimating the minimal lethal dose on the intact frog the drug was injected through the mouth into the breast lymph sac. In testing the action upon the isolated frog's heart the heart was perfused with a small quantity of Ringer's fluid containing the drug in solution, the method used being one described by Hartung[2] and slightly modified by myself.

In testing the action of the drug upon the isolated frog's sartorius, a method was used similar to that described by Dr. Waller; the isolated sartorius was suspended in Ringer's fluid and stimulated by strong induction shocks passed through the whole length of the muscle every ten seconds, the muscle was connected with a light lever, which was weighted with 2 grams, and the movements of the lever were recorded upon a slowly moving drum; the lever multiplied the movements of the muscle about ten times.



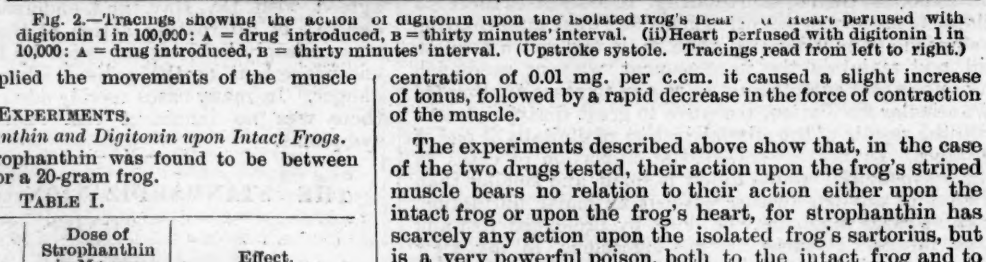


Fig. 2.—Tracings showing the action of digitonin upon the isolated frog's heart. (i) Heart perfused with digitonin 1 in 100,000: A = drug introduced, B = thirty minutes' interval. (ii) Heart perfused with digitonin 1 in 10,000: A = drug introduced, B = thirty minutes' interval. (Upstroke systole. Tracings read from left to right.)

EXPERIMENTS.

1. *The Action of Strophanthin and Digitonin upon Intact Frogs.*

The lethal dose of strophanthin was found to be between 0.014 mg. and 0.008 mg. for a 20-gram frog.

TABLE I.

No.	Weight of Frog.	Dose of Strophanthin.	Dose of Strophanthin in Mg. per 20-Gram Frog.	Effect.
	Grams.	Mg.		
1	11	0.022	0.04	Died in 2 hours.
2	14	0.014	0.02	Died in 2 hours.
3	14	0.007	0.014	Died in 2 hours.
4	14	0.004	0.008	Survived.

This result agrees with that of Famulener and Lyons,[4] who, using the same method, found the minimal lethal dose of strophanthin for a 40-gram frog was 0.02 mg. I did not attempt to ascertain the minimal lethal dose of digitonin, since Famulener and Lyons state that this drug has no apparent action upon a frog when injected subcutaneously.

2. *The Action of Strophanthin and Digitonin upon the Isolated Frog's Heart.*

The minimal concentration of strophanthin which would produce systolic arrest of an isolated frog's heart was estimated.

TABLE II.

No.	Concentration of Strophanthin in Mg. per C.cm.	Effect.
1	0.01	Systolic arrest in about 10 minutes.
2	0.001	Systolic arrest in about 30 minutes.
3	0.0005	Systolic arrest in about 60 minutes.
4	0.00025	No definite systolic action.

The action of strophanthin is shown in Fig. 1. It will be noted that the strongest doses produce no instantaneous effect, but that when systolic arrest occurs the arrest is incomplete, and no part of the heart continues to beat.

Digitonin also produces a systolic effect, as shown in Fig. 2, but much stronger concentrations are required to produce any effect. Digitonin in a concentration of 0.01 mg. per c.cm. of Ringer's fluid has no action upon the heart, but a concentration of 0.1 mg. per c.cm. produces a systolic effect; this effect, however, is entirely different to that produced by strophanthin, for the systolic effect in the case of digitonin is produced instantaneously, but is not complete, the auricles and part of the ventricles continuing to beat for some hours.

3. *The Action of Strophanthin and Digitonin upon the Isolated Frog's Sartorius.*

Strophanthin in concentrations of over 0.5 mg. per c.cm. of Ringer's fluid produced a gradual decrease in the force of the contraction—1 in 600 (Fig. 3), 1 in 2,000—but in a concentration of 0.01 mg. per c.cm. it produced no definite effect—1 in 100,000.

On the other hand, digitonin in a concentration of 0.1 mg. per c cm. caused immediate rigor of the muscle, and in a concentration of 0.01 mg. per c.cm. it caused a slight increase of tonus, followed by a rapid decrease in the force of contraction of the muscle.

The experiments described above show that, in the case of the two drugs tested, their action upon the frog's striped muscle bears no relation to their action either upon the intact frog or upon the frog's heart, for strophanthin has scarcely any action upon the isolated frog's sartorius, but is a very powerful poison, both to the intact frog and to isolated frog's heart; digitonin, however, has a marked action upon the isolated frog's sartorius, but has only a feeble action upon the isolated frog's heart, and, according to Famulener and Lyons, it has no demonstrable action upon the intact animal.

I therefore conclude that experiments upon the isolated striped muscle of the frog can be of no value for the physiological standardization of digitalis and allied substances, and that there is no necessary relation

between the physiological activity of a drug, measured by the response of striped muscle, and the toxic power, measured by the killing per unit-weight of living animal.

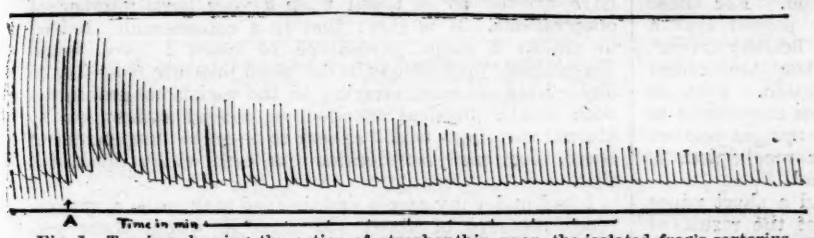


Fig. 3.—Tracing showing the action of strophanthin upon the isolated frog's sartorius. Sartorius suspended in Ringer's fluid, and stimulated every ten seconds with strong induction shocks. A = Strophanthus, 1 in 600, introduced. (Upstroke equals contraction. Tracing reads from left to right.)

The results obtained by Dr. Waller,[1] I think, may be easily explained by considering the nature of the drugs which he used; these drugs were as follows:

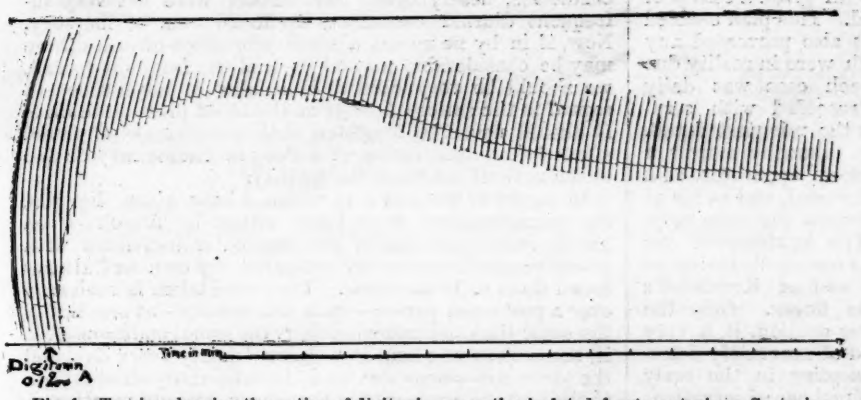


Fig. 4.—Tracing showing the action of digitonin upon the isolated frog's sartorius. Sartorius suspended in Ringer's fluid, and stimulated every ten seconds with strong induction shocks. A = Digitonin, 1 in 10,000, introduced. (Upstroke equals contraction. Tracing reads from left to right.)

1. Digitoxin (Merck).
2. Digitalin (Nativelle).
3. Digitalin (Merck).
4. Digitalinum verum (Schmiedeberg).
5. Digitalinum pulv. pur. German (Merck).
6. Tincture of digitalis.
7. A saponin.

Of these drugs, the first two had no effect upon striped muscle, but the last five all produced rigor in a concentration of 1 per 1,000. The first three drugs were all nearly insoluble, the fourth was soluble with difficulty, and the last three were readily soluble. The last five drugs all produced considerable frothing when their solutions were shaken.

Now digitoxin (Merck) and digitalin (Nativelle) both consist of nearly pure digitoxin, and this drug is soluble in 1 in 2,000 of water (Euler and Lundberg[5]). Dr. Waller found that a saturated solution of either of these preparations produced no action upon the isolated frog's sartorius, but I found that a solution of 1 in 700,000 of digitoxin produces systolic arrest of an isolated frog's heart[3]; and it is generally agreed that digitoxin is the most powerful of the glucosides isolated from digitalis.

It appears, therefore, that the glucosides digitoxin and strophanthin both have no action on the frog's striped muscle, but both have an extremely powerful action upon the frog's heart.

Five drugs produced rigor of frog's muscle, and four of these (Nos. 3, 5, 6, and 7), according to Kobert,[6] all contain large quantities of saponin, and the other drug (No. 4) probably also contains some saponin; for digitalin (Merck) is really a pure preparation of the saponin digitonine, and is the preparation used by the writer in the experiments already described. Digitalinum pur. pulv. German contains 60 per cent. of digitonin; tincture of digitalis always contains a considerable quantity of digitonin, and the last preparation used was a purified saponin. Digitalinum verum is said to consist of the pure glucoside digitalin; but this substance is less soluble than digitoxin and does not cause frothing, whereas the preparation used by Dr. Waller was more soluble than digitoxin and caused frothing, and therefore probably contained some digitonin (Euler and Lundberg[5]). Probably, therefore, the rigor of striped frog's muscle produced by the digitalis preparations used by Dr. Waller was due in every case to the presence of a saponin; these bodies have little action on the frog's heart, and there is no evidence that they are of any therapeutic value as cardiac tonics.

Therefore the action of digitalis preparations upon the isolated frog's sartorius, unfortunately, cannot be relied upon to give any indication as to the therapeutic value of the preparation.

Conclusions.

1. Strophanthin never produces rigor in the isolated frog's sartorius; in strong concentration it produces a gradual loss of the power of contraction.
2. Digitonin produces rigor in the isolated frog's sartorius, but has only a feeble action on the frog's heart; the effect produced is unlike that produced by the digitalis glucosides.
3. The action of preparations of digitalis in producing rigor of the isolated frog's sartorius is probably due entirely to the digitonin or other saponins present, and is not due to the glucosides.
4. There is no necessary relation between the action of preparations of digitalis and allied substances upon the striped muscle of the frog and their action upon the frog's heart.

REFERENCES.

[1] Waller, *Journ. of Phys.*, 38, 1909; *Proc. Phys. Soc.*, x. [2] Hartung, *Arch. f. exper. Path. und Pharm.*, 3, 66, S. 1, 191. [3] Clark, *Proc. Roy. Soc. Med.*, June, 1912. [4] Famulener and Lyons, *Chem. Centralbl.*, 1903, i, 415. [5] Euler and Lundberg, *Biochem. Handlexikon* (Aberhalben), 1911, B. ii, S. 578. [6] Kobert, ibid., B. vii, S. 145.

SOME INVESTIGATIONS OF THE ACTION OF DIGITALIS ON THE BLOOD PRESSURE IN MAN.

By FREDERICK W. PRICE, M.D., M.R.C.P.,

Physician to the Great Northern Hospital and to the Mount Vernon Hospital.

AMONG the common beliefs which are held without questioning by the profession at large is the belief that the internal administration of digitalis increases the blood pressure in man by constricting the peripheral vessels. This view is found in the textbooks of pharmacology and therapeutics, and it is taught in the medical schools. The result of it is that therapeutists are afraid of administering the drug in cases of cardiac failure in which there is extensive degeneration of the arterial coats, for fear of rupture of their walls and apoplexy, or where cardiac dilatation is associated with increased arterial tension from disease of the arterial coats and of the kidneys.

Some try to get over the difficulty by prescribing along with digitalis some appressor, such as sodium nitrite or nitroglycerine, in order to dilate the vessels and lessen the arterial tension. It should not be forgotten, however, that the effects of appressors are but transient, while the rise in the peripheral resistance—if it is brought about by digitalis — is practically continuous. Others employ strophanthus instead in these cases, because of the prevailing view that it constricts the peripheral vessels less or not at all. Now, the belief that digitalis raises the blood pressure by causing arterial constriction rests, with the exception of Sir Lauder Brunton's observations by means of the sphygmograph upon himself, entirely upon experiments on animals, and animals which are in normal health. The experimentalist, in order

to get his results within a few hours, usually employs extremely large doses. Also, the reaction of a drug is influenced by the nature of the lesion. For these and other reasons, although it may be proved that a particular drug has a certain action on a healthy animal, it does not necessarily follow that it has the same action on a human being with a diseased condition. Also, in regard to blood pressure, it is of the utmost importance to remember that in man, in order that the upright posture may be maintained, the vaso constrictor mechanism is much more perfect than in the lower animal.

At the annual meeting in 1911 I read a short paper which embodied in a practical form some of the results of a series of investigations at the Mount Vernon Hospital, dealing in detail with the action of digitalis on the cardio-vascular system in man, conducted by Dr. James Mackenzie, Professor Cushny, myself, and others. In regard to the methods of investigation, with the exception of those cases where the symptoms required immediate treatment, all the patients were first of all given a course of rest before any drug was administered. This plan enabled us to study the effects of rest, and it also prevented any drug being credited with results which were in reality due to adventitious circumstances. Each case was daily observed, examined, analysed, and recorded with scrupulous care, attention being paid to the minutest detail. Blood pressure measurements were taken by means of Martin's modification of the Riva-Rocci apparatus, as a rule in each case over a prolonged period, and as far as possible the same individual took records of the same cases throughout their whole course. The accuracy of our sphygmomanometric observations was repeatedly tested by the employment of the polygraph, and of Korothloff's auscultatory method instead of the finger. Only the systolic pressure was taken, as, in our opinion, it is very doubtful if as yet we have a method of accurately determining the diastolic pressure. Excepting in the early stages of our investigations we confined our observations to cases with a regular heart action or with an occasional irregularity—that is, where there were periods of regularity of sufficient length during which to take measurements. In hearts with a completely irregular rhythm—in other words, in cases of auricular fibrillation—where there is a constant variation in the strength of the pulse beats, the pressure in the air-bag will obliterate some of the beats, and may not obliterate some of the other beats, and as the beats come through in an irregular fashion, in our opinion it is impossible to get a figure which represents the condition, and therefore we came to the conclusion that the records of cases of auricular fibrillation were not sufficiently reliable to be of much value.

In regard to digitalis, our practice was, as a rule, to give a drachm of the tincture a day, either 15 minims four times a day, or 20 minims three times a day; but in regard to the cases in which blood pressure measurements were carefully noted we sometimes gave only 15 minims three times a day. In practically all the cases these doses were continued until some definite reaction was obtained, such as vomiting and heart irregularities.

The cases in which blood pressure observations were carefully made were: Seven cases of mitral disease, one case of mitral disease with a systolic murmur over the sternum, one case of cardio-sclerosis and arterio-sclerosis, four cases of double aortic and mitral systolic bruit, and two cases of double mitral and double aortic disease. In all excepting five cases there was no observable change in the blood pressure during the administration of the drug. Of these five cases, in three there was a fall equal to about 20 mm. of mercury towards the end of the period of administration (in one of these the fall remained after the drug was stopped); in one there was probably a fall; and in one there was a rise equal to about 20 mm.—namely, in a case of double aortic disease and mitral systolic murmur, in which there was oedema, ascites, and impairment and râles over the bases of the lungs. A short abstract of all the cases is appended.

It seemed to me that the matter was of such great practical importance that I decided to extend these observations, and for nearly a year now I have been conducting investigations of blood pressure in man by the methods in use for observing it clinically. In regard to the general subject of blood pressure in man, one important point has been elicited which has a very important bearing upon the question of the possible action of digitalis. In this respect, I may say that the conclusion which I have arrived at is based upon a very large number of observations. It is this: that in a considerable, indeed in almost a large, percentage of cases I have found considerable fluctuations in the blood pressure from day to day. I am not now referring to the variations associated with meals, physical exertion, or mental states; but to diurnal variations which appear to be quite independent of these. Let me just mention one case which illustrates this.

I had under my care a middle-aged man, with a systolic blood pressure of about 150 mm., in whom there was a moderate degree of thickening of the vessel wall, and a slightly enlarged heart, but no evidences of kidney disease. He was under my observation in the hospital for five months. For many weeks I kept him in bed and took his blood pressure myself, with the same instrument, at the same time of the day, and under precisely the same conditions, nearly every day. There were not very infrequent diurnal variations, up to 26 mm. of mercury. Now, if in by no means a small proportion of cases there may be considerable normal fluctuations from day to day, we should be very careful in coming to conclusions in regard to the action of drugs on the blood pressure in man. It should never be forgotten that any changes observed after the administration of a drug in disease may be due to the natural course of the malady.

In regard to the cases in which I have given digitalis, the measurements were taken either by myself or on nearly every occasion by one particular individual with whose results I repeatedly compared my own and always found them to be the same. They were taken in each case over a prolonged period—up to five months—at practically the same time and under precisely the same conditions (that is, the patient was kept in bed, etc.) nearly every day, and the same instrument was used. I confined my observations to the systolic pressure for the reason already mentioned. Excepting where otherwise stated, I gave a drachm of the tincture per day. In practically all the cases these doses were continued until some definite reaction was obtained. Five cases have been recorded with the greatest detail. In none was there a rise in the blood pressure during the administration of the drug, and in one possibly there was a fall. I will read a short abstract of each case.

CASE I.

A labourer, 52 years of age. Complaint, pain in the precordia. Former history of excessive beer drinking. Accustomed to very severe physical exertion. No history of rheumatism, chorea, or scarlet fever. Slight tuberculous lesion left apex detected three years ago; soon became quiescent. Pain in precordia the last three years. Dyspnoea on exertion two years, present on lying down last few months.

State on Admission.—A short, thick-set, muscular man. Tenderness of left breast, sterno-mastoid, and trapezius. Pulse of normal rate and rhythm. Apex beat in the fifth interspace, 4½ in. to left of middle line. Faint mitral systolic bruit. Aortic second sound considerably accentuated. Evidences of healed lesion left apex, and of emphysema. Palpation reveals a considerable degree of thickening of wall of radial artery. From six weeks onwards after admission for ninety-nine days the average systolic blood pressure was 152 mm. It showed a considerable range of diurnal variation, from 134 to 160, apart from one occasion, when it was 170. During this period of ninety-nine days three courses of tincture of digitalis were administered—namely, (1) for nine and a third days, (2) for nine days, and (3) 30 min. t.i.d. for five days.

On comparing the blood pressure records of the periods when the patient was not taking the drug with those when the drug was being administered, it cannot be said that the blood pressure became raised during the latter.

CASE II.

A carpenter, 36 years of age. Complaint, "rheumatism." Acute rheumatism at 16, since when he has suffered from slight dyspnoea and palpitation on exertion. Has had subacute rheumatism the last seven weeks.

State on Admission.—No pyrexia. Pulse between 70 and 80, of normal rhythm. Apex beat in the fifth interspace, area and force considerably increased. Transverse diameter of heart 5⅝ in. Loud, rather harsh, mitral systolic, with unusually large area of propagation. Wall of radial artery not unduly palpable. Systolic blood pressure ranges from 143 to 160, excepting that on one occasion it was 136, the average being 147. Two course of tincture of digitalis were administered—namely, (1) for nine days and (2) for six days. No change in blood pressure observed.

CASE III.

A bricklayer, 59 years of age. Complaint, attacks of faintness and pain in the region of the heart. No history of excess of

Age.	Sex.	History.	Pulse.	Heart, etc.	Blood Pressure.	Drug.	Change in Blood Pressure.	Change in General Condition.
21	F.	None of rheumatism or chorea	80-90, normal rhythm	Apex beat 5th interspace; transverse diameter 6½ in.; mitral presystolic, and sometimes diastolic	110-120 mm.	3j per day for 7 days	Fall of about 20 mm. by end of period of administration	Second course of drug followed by auricular fibrillation for 4 days; during 3rd course heart-block occurred.
23	M.	Scarlet fever; none of rheumatism	90-100, occasional extrasystole	Apex beat 5th interspace; transverse diameter 5 in.; mitral presystolic and diastolic	106-228 mm.	3j per day for 7 days	None	Drug caused heart-block.
30	F.	Acute rheumatism thrice; cerebral embolism	50-70, normal rhythm	Apex beat 6th interspace, ¾ in. outside nipple line, and forcible; mitral presystolic and occasionally diastolic	90-100 mm.	(1) m45 per day for 9 days, (2) 3j per day for 6 days	None	Slight decrease of pulse-rate
34	F.	None of rheumatism	60-70, normal rhythm	Apex beat 6th interspace, outside nipple line; mitral presystolic	112-128 mm.	3j per day for 5 days	Certainly no rise	No apparent effect on heart.
36	M.	Acute rheumatism four times	About 100, normal rhythm	Transverse diameter 3 in.; short mitral presystolic	116-134 mm.	3j per day for 5½ days	Fall of about 20 mm. by end of period of administration	Drug caused partial heart-block, but did not materially slow the pulse.
32	F.	Acute rheumatism thrice	80-90, normal rhythm	Transverse diameter 6½ in.; mitral presystolic, systolic, and diastolic	About 100 mm.	m45 per day for 8 days	None	No effect upon the heart.
50	F.	Acute rheumatism thrice	96, frequent extrasystole, and pulsus alternans after each	Left border 4½ in. from mid-sternal line; mitral presystolic and systolic	142-172 mm.	m45 per day for 5 days	Certainly no rise	No effect upon heart-rate or upon occurrence of extrasystoles.
24	F.	Scarlet fever, acute rheumatism, and chorea	70-80, normal rhythm	Transverse diameter 7¾ in.; mitral presystolic and diastolic; systolic over sternum	112-130 mm.	3j per day for 11¾ days	Certainly no rise; probably a fall	Decreased heart-rate and caused heart-block, sinus irregularity, extrasystoles, and pulsus alternans.
16	M.	None	120-140, normal rhythm, collapsing	Apex beat 5th and 6th interspace, outside nipple line, diffuse; aortic diastolic, and mitral systolic; bases impaired, and fine râles; liver large and pulsating; ascites; much oedema	116-134 mm.	(1) 3j per day for 15 days, (2) 3j per day for 9 days	None	Produced heart-block, extrasystoles, and pulsus alternans; it increased the flow of urine, but had no effect upon the auricular rate.
41	M.	Acute rheumatism thrice	About 90, normal rhythm, collapsing	Arterial wall thick and somewhat tortuous; apex beat 6th interspace, diffuse; transverse diameter 9¾ in; mitral systolic; double aortic; cyanosis; orthopnoea; slight jaundice; much oedema; slight albuminuria	170-180 mm.	3j per day 7 days after admission for 7 days	Rise of about 20 mm.	Caused diuresis; pulse did not decrease in rate.
20	F.	Acute rheumatism several times	About 100, normal rhythm, collapsing	Apex beat 6th interspace, diffuse; transverse diameter 7¾ in.; mitral systolic; double aortic	120-154, average 138, mm.	(1) 3j per day for 5 days, (2) m45 per day for 11 days	None None	No effect upon the heart-rate.
46	F.	Rheumatism each winter, but no acute attack	80-90, normal rhythm	Apex beat 6th interspace, 1½ in. outside nipple line; mitral systolic; double aortic; orthopnoea; oedema	130-140 mm.	3j per day for for 18¾ days	Fall of 20 mm., which remained after drug stopped	Practically no effect upon the heart.
18	M.	Acute rheumatism several times, severe physical exertion	100-110, normal rhythm	Apex beat 6th interspace; transverse diameter 5 in.; mitral systolic; double aortic; liver large; orthopnoea	124-136 mm.	3j per day for 5 days	None	No effect upon the heart.
46	F.	—	80-90, anacrotic, occasional extrasystole	Apex beat 6th interspace; transverse diameter 6½ in.; mitral presystolic and diastolic; double aortic; emphysema	107-135, average 117, mm.	3j per day for 4½ days	None	No apparent effect upon the heart.
54	F.	None of rheumatism	80-90, occasional extrasystole	Left border 5 in. to left of mid-sternal line; mitral presystolic and diastolic; double aortic; emphysema; attacks of semi-unconsciousness	96-125, average 106, mm.	3j per day for 4½ days	None	Slight improvement in the general condition, but no appreciable effect upon the heart.
39	M.	Scarlet fever, acute rheumatism, and influenza many times	About 70, normal rhythm	Left border 5½ in. to left of mid-sternal line; mitral systolic; aortic systolic	100-118, average 111, mm.	3j per day for 7 days	None	No apparent effect upon the heart.

alcohol or of venereal disease. Increasing dyspnoea on exertion last three years. Dull, heavy pain in precordia on exertion last two years, worse since April, 1911; recently even on walking on level, and occasionally after getting into bed following exertion; when attack is severe, pain sometimes radiates to right shoulder and down right arm. When attacks severe sense of constriction round chest very definite, and on few occasions has had sensation of impending dissolution. Sometimes falls down during attacks, followed by retching.

State on Admission.—Pulse between 60 and 70, of normal rhythm. Tenderness of left breast, sterno-mastoid, and trapezius. Apex beat in fifth interspace. Left border of heart, 3¾ in. from mid-sternal line. Faint localized mitral systolic. No aortic systolic, but second sound rather accentuated. Wall of radial artery considerably thickened and tortuous. Systolic blood pressure ranges from 104 to 120, being usually about 110, and the average being 111. Tincture of digitalis was given for about a fortnight. Certainly no increase in blood pressure, possibly a fall.

CASE IV.

A carman, 37 years of age. Complaint, pain in the epigastrium and left arm on exertion. Wassermann reaction positive. Accustomed to very severe physical exertion. History of quinsy; none of acute rheumatism, chorea, or scarlet fever. Has suffered from angina pectoris for three and a half months; attacks now occur on least exertion.

State on Admission.—Unusually well-developed, muscular man. Pulse of normal rate and rhythm. On examination of the heart absolutely nothing abnormal detected (no aortic systolic bruit or accentuation of second sound). Knee-jerks feeble. Urine normal. Wall of radial artery not unduly palpable. Systolic blood pressure ranges from 110 to 130 (except that one occasion 140), the average being 120. Digitalis administered for ten days. No change in blood pressure observed.

CASE V.

A male, 45 years of age, formerly a machinist (which involved rather severe physical exertion), last seven to eight months a paperer. Complaint, shortness of breath on exertion. No history of alcohol or venereal disease. Rheumatic fever at 34. Influenza, November, 1910. Dyspnoea and palpitation on exertion since Christmas, 1910.

State on Admission.—A spare man. Pulse 140, tracing reveals pulsus alternans. Apex beat in the fifth interspace, area and force moderately increased. Left border of heart 4¾ in., and right border 2½ in., from middle line (right border of heart came in nearly 1 in. latter). Aortic systolic and diastolic. Mitral diastolic. Liver palpable. Moderate emphysema. Urine normal. Wall of radial artery not unduly palpable. Systolic blood pressure varied from 134 to 146. A course of digitalis was administered. No rise in blood pressure.

The conclusions I draw from these investigations are as follows: Taking the twenty-one cases, in regard to which I venture say that thoroughly accurate observations were made, in only one case was there a rise in blood pressure. When it is remembered that in several cases there was a *fall* of pressure, and especially when we remember the fact that, as I have pointed out, not infrequently a normal diurnal variation of considerable range occurs, I think we may say, that so far as these cases are concerned, judged by the methods in use for observing the blood pressure clinically, the internal administration of digitalis does not raise the blood pressure in man by constricting the peripheral vessels. If that be so, the very important practical point will have been discovered that there is no risk in administering the drug in cases of degeneration of the walls of the blood vessels or of supernormal blood pressure. It may be said that the number of the cases which I have investigated is not large. But when we consider the long-continued and detailed observations in each case, I am inclined to think I am justified in coming to the conclusion I have named.

CLINICAL EXPERIENCES WITH TETRAHYDRO-PAPAVEROLINE HYDROCHLORIDE.

By C. R. MARSHALL, M.D., M.A.,

Professor of Materia Medica and Therapeutics in the University of St. Andrews; Physician to the Dundee Royal Infirmary.

THIS alkaloid was prepared by Pyman[1] and was investigated physiologically by Laidlaw.[2] Laidlaw found that the substance inhibits the tone of unstriped muscle fibre, especially that of the blood vessels, the bronchioles, and the uterus, and stimulates the heart. Both by perfusion experiments and by cardiometric tracings with an intact circulation he showed that the rate of the heart-beats was enormously increased and their force considerably augmented. Notwithstanding this effect, the intravenous injection of 3–10 mg. to anaesthetized animals caused a rapid and marked fall of blood pressure; in one case an injection of 3 mg. induced a fall of 70 mm. Hg. On the bronchioles the effect was also powerful; 4 mg. counteracted the constricting effect of 1 mg. pilocarpine injected previously, and prevented the action of 10 mg. pilocarpine subsequently. The substance was also found to be relatively non-toxic; 25 mg. given hypodermically to a cat produced no obvious symptoms beyond a slight increase in the frequency of the pulse, and 50 mg. by the mouth half an hour later merely caused a further increase in the pulse-rate.

The relative non-toxicity of the substance and the combined effects of vaso-dilatation, relaxation of the bronchioles, and stimulation of the heart, produced by it, suggested that it might be of value as a therapeutic agent, and at the request of a representative of Messrs. Burroughs, Wellcome and Co. I agreed to give the new drug a trial. For this purpose a gross of tabloids, each containing ½ gr. (the suggested dose) of tetrahydro-papaveroline hydrochloride, was kindly sent to me by the firm.

The pharmacological experiments seemed to indicate that the substance might be useful, at least as a palliative, in the treatment of arterio-sclerosis with commencing heart failure, and possibly also in dyspnoeic conditions and heart failure due to other causes. Its administration was consequently confined to this class of cases. Before commencing its administration I took two tabloids, and half an hour later four tabloids, with no other effect than slight fullness in the head and slight and transient increase in the frequency of the pulse from ten to twenty minutes afterwards.

The cases in which the drug was administered were four in number. Three had arterio-sclerosis associated with chronic Bright's disease, and had a blood pressure of over 200 mm. Hg; the fourth was a case of chronic myocarditis and pericarditis (adherent pericardium) with marked emphysema. The drug was given in doses of 1 grain (two tabloids) three times a day, and its effects compared with those of one or more members of the nitrite group. Thus administered, it produced no obvious effect on the form of the pulse tracing, and it had less effect on the blood pressure than 2-grain doses of sodium nitrite or half-grain doses of erythrol tetranitrate. It also failed to prevent the attacks of dyspnoea to which two of the arterio-sclerotic patients were subject, or to relieve the difficulty of breathing in the case of cardiac failure. But this was equally true of erythrol tetranitrate. The attacks, however, were relieved by inhalation of amyl nitrite, and usually, but less completely, by 1 drachm of sweet spirit of nitre in water by the stomach. The three arterio-sclerotic patients expressed themselves as feeling at first more comfortable under the influence of the drug, but a second administration in one case when symptoms of heart failure began to be more manifest failed to give any relief. No ill effects from the drug were observed or complained of except in the heart case, in whom it seemed to induce nausea, and its administration had to be discontinued in consequence. On the whole, considering the pharmacological action of the drug after intravenous injection, its therapeutic effects were disappointing. This may be due to the employment of insufficient doses, but is more probably due to the ease with which it oxidizes. Appended is a brief account of the cases in which the drug was tried.

CASE I.

Woman, aged 51. Engaged in household duties. No previous illnesses. The present illness commenced ten months before admission with breathlessness, palpitation, and cough. Later headache, nausea, and vomiting appeared. Under treatment she improved from time to time, but the symptoms always reappeared, and the coughing, which was often spasmodic in character, sometimes induced vomiting of dark masses of blood.

When admitted under the care of my colleague, Dr. Mackie Whyte, she was suffering from headache, especially in the morning, and breathlessness. The heart was much enlarged—the apex beating 5½ in. from the middle line—and a mitral systolic murmur and an accentuated second sound, especially in the aortic area, were present. The pulse was irregular both in force and rhythm, and the maximal blood pressure was 270 mm. Hg. There were slight dullness and some crepitations at the bases of both lungs, and a trace of albumen and a few hyaline and granular casts were present in the urine. With rest in bed and the administration of caffeine for the headache, the patient improved somewhat. Later 2 grains of sodium nitrite three times a day was ordered, but was discontinued four days later owing to a fainting attack with headache, giddiness, sickness and palpitation, occurring while in bed. A few days later the case came under my notice, and at my suggestion Dr. Whyte kindly consented to give the new drug a trial. The administration was commenced six days after the discontinuance of the sodium nitrite, two tabloids (1 grain) being given three times a day. The following table shows the effect of the first dose of 2 grains sodium nitrite and of 1 grain tetrahydro-papaveroline hydrochloride on the maximal blood pressure:

Time.	Blood Pressure in mm. Hg.	
9 a.m.	250	230
9.30 a.m.	2 grains sodium nitrite	1 grain tetrahydro-papaveroline hydrochloride.
10 a.m.	200	210
11 a.m.	220	210
1 p.m.	230	220

The morning following the four days' treatment with sodium nitrite the maximal blood pressure was 190 mm. Hg; the morning after the tetrahydro-papaveroline treatment was discontinued it was 208 mm. Hg. The day after the commencement of the new drug and the two following days the patient said she felt much better, but on the fourth day, notwithstanding the administration of the medicine, she complained of headache and discomfort. There was no decided influence on the pulse. The pulse tracings show no change until the end of the four days' treatment, when longer periods of regularity occurred.

Eight days later the tetrahydro-papaveroline treatment was recommenced. The maximal blood pressure was then 202 mm. Hg. The first 1-grain dose of the drug caused a fall to 180 mm. Hg. forty-five minutes after its administration. The following day she again expressed herself as feeling better, but the third night she had an attack of breathlessness. She was ultimately discharged much relieved.

CASE II.

A well-developed, intelligent man, aged 57, a bleachfield worker. Work formerly arduous, latterly not so. During the last few years had taken considerable quantities of alcohol. History otherwise unimportant. Three months before admission he ceased work owing to breathlessness occurring on exertion and palpitation occurring after retiring to bed at night. Shortly afterwards precordial pain appeared; it came on suddenly, lasted a few minutes, and disappeared suddenly. Some puffiness around the eyes was also noticed, and was followed by swelling of the legs and by headache. Under treatment slight improvement occurred, but he was eventually admitted to the Dundee Royal Infirmary.

On admission he complained of breathlessness and occasional precordial pain. There was slight difficulty in breathing and slight puffiness about the eyes, but no other oedema. The heart was much enlarged, the apex beating 6½ in. from the middle line, and a rough blowing systolic murmur, slightly more marked over the sternum than elsewhere, was heard over the greater part of the chest in front and behind. The pulse was regular, 90 per minute, and of high tension. The blood pressure, measured by Martin's instrument, was 280 mm. Hg. A few rhonchi were heard in the chest, and some slight dullness and some crepitations were present at the bases of both lungs. There was a trace of albumen in the urine.

Four days after admission, during which time he had improved slightly, 1 grain tetrahydro-papaveroline hydrochloride three times a day was ordered. This treatment was continued five days. Two days after its commencement he said he felt better, and at the end of the treatment the breathing was less embarrassed, but the improvement did not seem to be greater than that which occurred during the four days prior to the treatment. The blood pressure was not greatly influenced. The four days' rest in bed had reduced it to 260 mm. Hg. After cessation of the papaveroline treatment it was 240 mm. Hg. The fall induced by any single dose did not amount to more than 10 mm. Hg. Two days after stopping the treatment he complained of breathlessness, and the following day he had an attack of dyspnoea. The three following days were spent in comfort. Then 1 grain pentaerythrite tetranitrate (a compound somewhat less active than erythrol tetranitrate) was ordered every eight hours. It caused headache at first, and on the fourth and fifth days of the administration some difficulty in breathing was complained of. Otherwise the patient was comfortable during the eight days this treatment was continued. The lowest blood pressure recorded during this time was 216 mm. Hg. At the end of the treatment the blood pressure was 240 mm. Hg. Later the breathing again became embarrassed and occasionally attacks of dyspnoea occurred during the night. For these attacks inhalation of amyl nitrite was ordered, and almost invariably gave relief. On a few occasions morphine was administered. Potassium iodide and subsequently pentaerythrite tetranitrate were given without benefit, but digitalin night and morning, with a drachm of sweet spirit of nitre three times a day, produced improvement. The patient was in hospital for eighteen weeks. For ten weeks the blood pressure, apart from variations induced by the vaso-dilators, remained about 230 mm. Hg. Thirteen weeks after admission the heart commenced to fail, the breathing became more embarrassed, and oedema appeared in the legs. Tetrahydro-papaveroline hydrochloride 2 grains (four tabloids) night and morning failed to produce any beneficial effect. There was no improvement in the breathing or diminution of the dyspnoeic attacks, and no stimulant action on the heart. The fall in blood pressure produced by this dose at this time was only 4 to 6 mm. Hg. Digitalis given subsequently produced temporary improvement, especially in the breathing, and ½ grain of erythrol tetranitrate night and morning given after the digitalis was stopped also produced some degree of comfort and a greater fall of blood pressure (8 mm. Hg) than the 2 grains of tetrahydro-papaveroline hydrochloride. From the present point of view the subsequent course of the case was disturbed by the presence of a duodenal ulcer which ultimately perforated.

CASE III.

Male patient, aged 60. Present illness commenced a year before admission, with breathlessness and palpitation on going up a hill. Later he had at times severe precordial pain passing to the left shoulder and down the left arm. When admitted the heart was found enlarged—the apex was 5 in. from the middle line—and beating irregularly. The sounds, especially the aortic second, were accentuated, but no murmurs were present. The maximal blood pressure was 210 mm. Hg. There was no albumen in the urine.

With rest in bed he improved considerably. Subsequently tetrahydro-papaveroline hydrochloride, 1 grain three times a day, was given without apparent benefit, and later, digitalis, pentaerythrite tetranitrate, and erythrol tetranitrate were administered in succession with better effect. The results were similar to those obtained in the earlier stages of the previous case. The patient was discharged much improved ten weeks after admission.

CASE IV.

Male, aged 46, suffering from cardiac failure. There was marked breathlessness, orthopnoea, some cyanosis and oedema, and troublesome cough with moderately profuse expectoration. The heart was much enlarged, and there was marked emphysema. (*Post mortem* the pericardium was found to be adherent.)

Under treatment he improved at first, but seven weeks after admission he commenced to get worse, and died seven and a half weeks later. Eight weeks after admission 1 grain tetrahydro-papaveroline hydrochloride three times a day was ordered in the hope that it would stimulate the heart. It had no such effect. The pulse and blood pressure, which was low (100 mm. Hg), were not materially influenced, and the embarrassed breathing was not relieved. On the first day of its administration he complained of sickness, and about midnight vomited. For the next two days he felt sickly at times, but did not actually vomit, and, as he was convinced that the nausea was due to the medicine, its administration was discontinued. The nausea then disappeared. This is the only occasion on which I have noticed ill effects coincident with the administration of this drug.

REFERENCES.

[1] *Trans. Chem. Soc.*, xcv, p. 1610, 1909. [2] *Journ. Physiol.*, xl, p. 481 1910.

WEST INDIAN CANE SUGAR IN THE TREATMENT OF CERTAIN FORMS OF HEART DISEASE.

By ARTHUR GOULSTON, M.A., M.D.Cantab.

IN this paper I wish to bring before you a suggestion bearing on the treatment of heart disease by means of cane sugar.

In the pages of the BRITISH MEDICAL JOURNAL there have been brought before the notice of the medical profession—by myself and others—a paper and two cases on the subject of the specific action of cane sugar (West Indian) on the myocardium in auricular fibrillation, which is present, according to Dr. Thomas Lewis and Dr. James Mackenzie, in from 60 to 70 per cent. of all failing irregular hearts. In the BRITISH MEDICAL JOURNAL for July 13th, 1912, Dr. Thomas Lewis says:

> In 60 to 70 per cent. of all failing hearts, irregularity is the result of a remarkable phenomenon which I described in this JOURNAL three years ago—namely, fibrillation of the auricles.

Now, I first brought this specific action of cane sugar (West Indian) on the failing myocardium before the notice of the medical profession generally in a short article I published in the BRITISH MEDICAL JOURNAL of March 18th, 1911. For certain reasons I had to write this article in a great hurry; otherwise I should have mentioned the fact that I had been using cane sugar for these irregular, failing hearts with considerable success for the previous eight or nine years. In consequence of the publication of this little article two striking cases have been published in the BRITISH MEDICAL JOURNAL—namely, one by A. H. Carter, M.D.Lond., on November 25th, 1911, and the other by Dr. Dingle on January 13th, 1912. In my thesis for the degree of M.D. at Cambridge, read May 9th, 1912, I established the fact that the ingestion of cane sugar has a beneficial effect on the myocardium in certain forms of heart disease. Now, in my hands and in the hands of my numerous correspondents this mode of treatment has been eminently successful; and in a large proportion of cases the improvement in the state of the heart and of the patient has been permanent. I have made these preliminary remarks in order to show that the treatment is being made use of by many medical men, and that, therefore, it is to that extent an acknowledged mode of treatment. The sugar treatment is no panacea for diseases of the heart, but it is a valuable addition to our therapeutic armament against failing, irregular hearts.

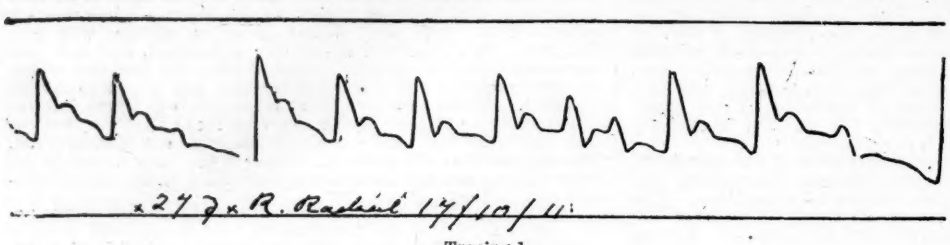
Given this premiss, I wish to make a suggestion about the mode of action of the temporary excess of dextrose, derived from the ingestion of cane sugar, in the blood; that is to say, does the nutritional value of the dextrose for the myocardium depend on the dextrose alone—as would seem to be the case from Dr. F. S. Locke's experience—or does it depend upon the introduction into the circulation of dextrose *plus* something else? The idea

that there might be another factor in the process was aroused in my mind by the following two cases:

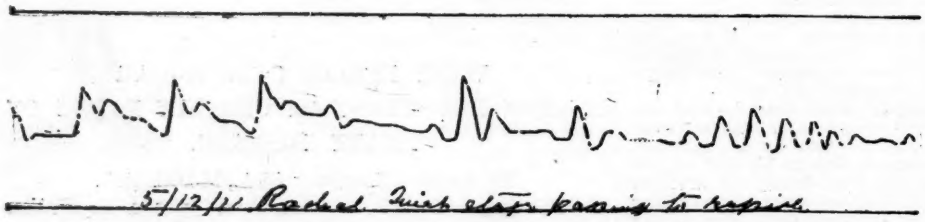
1. Lady, 79 years of age, one of my early successful cases. She had given up taking sugar except in her tea. From my point of view, this means giving up the treatment. In October, 1910, I was called in to see her, and I found her heart irregular again and feeble, and she was feeling ill. I advised her to return to her cane sugar treatment for a time. I saw her again in a week's time, but there was no improvement. At the end of the next week, as there was still no improvement, I thought I would make a few inquiries. I discovered that she was taking beet sugar, and not the sugar made from the sugar cane—that is, West Indian sugar. I then took means to ensure that she took the particular cane sugar which I know to be absolutely pure. At the end of another week the heart was much better, and in a fortnight it was quite regular and the patient had recovered her old feeling of well-being.

2. A gentleman, 53 years of age, consulted me towards the end of May, 1912. I made up my mind that his was a suitable case for the treatment. I put him on it, and he improved rapidly, and after six weeks' treatment he was practically quite recovered and was in his former good health. This patient had to give up hunting on account of his severe heart symptoms, but now he can ride and and walk up the stiff hills in North Devon.

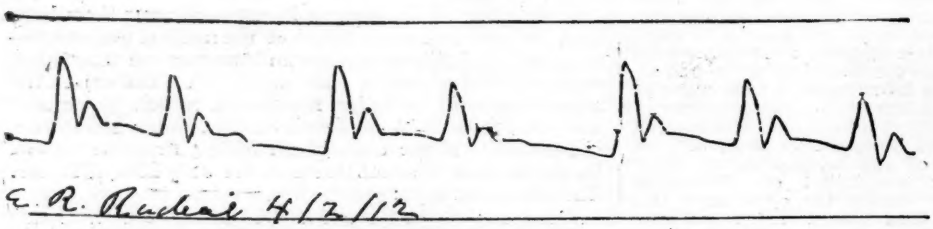
He dates the commencement of his heart trouble from about twelve months ago. He struggled on, thinking that his health would improve, until in December last he had to give up hunting, a sport to which he is devoted. Now comes the point I wish to emphasize. When the first bag of pure cane sugar (West Indian) arrived his wife immediately said, "This is the same kind of sugar we used up to twelve months ago," and, in proof of her statement, she sent for one of the old bags which had been given to the gardener to keep his seeds, etc., in. As I have said, this patient dated the commencement of his heart trouble to about twelve months ago—that is to say, he began to notice that he could not do quite so much as he had been in the habit of doing on account of his breathlessness and palpitation on exertion (even slight), till finally he was obliged to give up hunting altogether. Now, directly the ingestion of cane sugar (West Indian) was resumed—under my treatment—he began to mend, and in the course of six weeks became quite well and able to do anything reasonable in the way of walking uphill and riding. When the cane sugar (West Indian) was given up a year ago beet sugar was used in its place until I put my patient on the cane sugar (West Indian) treatment.



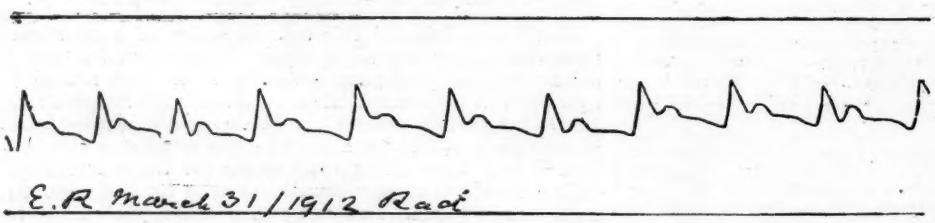
Tracing 1.



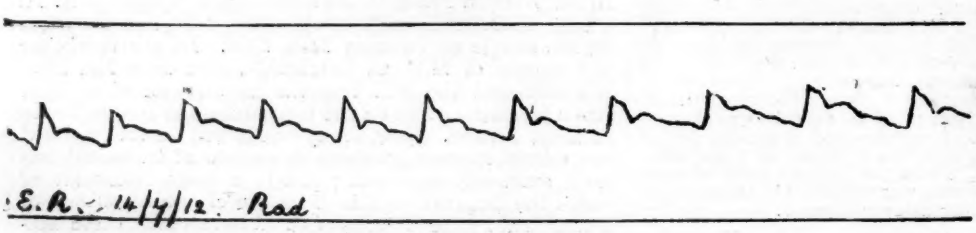
Tracing 2.



Tracing 3.



Tracing 4.



Tracing 5.

Now, in these two cases their hearts failed directly the beet sugar was taken instead of the cane sugar (West Indian), and rapidly improved when the process was reversed and the cane sugar (West Indian) was substituted for the beet sugar.

I naturally ask myself, Why is this? I venture to suggest that there is "something" in cane sugar (West Indian) which is not present in beet sugar. What is it? Is it of the nature of a co-enzyme or activator, or what?

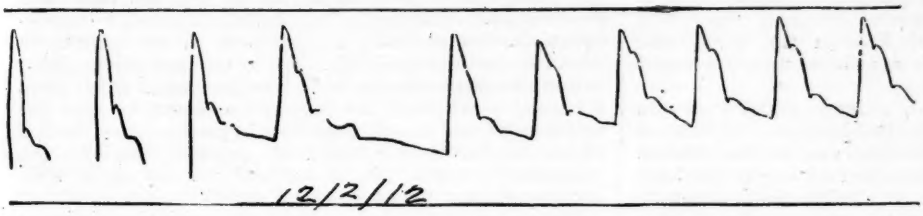
Now, I believe I am right in saying that up to the present time cane sugar, regarded from the purely chemical point of view, is cane sugar whatever its source may be, so I do not get much help from the chemists; but, in spite of this, I ventured to bring my idea to the notice of three of the leaders in the chemical and physiological world who have given me much help in the past, and they all agree that, although they do not know of anything of the kind, it is not inherently impossible that there may be a factor in cane sugar (West Indian) which is not present in beet sugar. As these physiological and chemical giants did not pooh-pooh my clinical inference, I was able to summon up enough courage to bring the point before your notice as I have now done.

Before closing this part of my paper I should like to mention a curious fact in connexion with the feeding of bees which is interesting in this connexion. Practical bee-keepers tell me that it is well known that the syrup used for feeding bees in winter must be made with West Indian sugar, for if it is made with beet sugar the bees deteriorate, and many contract disease and die. Here, again, it would seem that there is a something in the West Indian cane

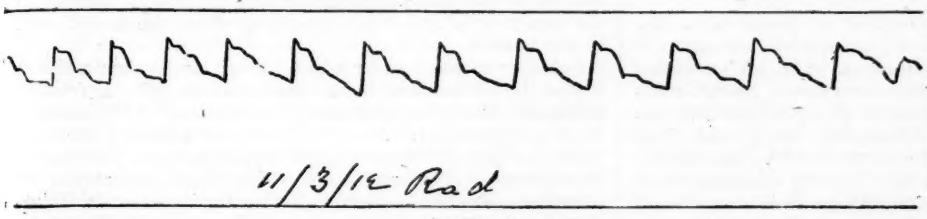
sugar which has a beneficial effect on the bees but which is not present in beet sugar.

The patient from whom tracings 1 to 5 were taken, a young man of 28 years of age, has been under my cane sugar treatment since November, 1911. He had been treated in the hospital at Exeter for his paroxysmal tachycardia for rather more than two years before he came to me in November, 1911, but without benefit. He had spent periods of many months in hospital, and when not an in-patient was treated as an out-patient.

As he could not get another "recommend" for the hospital, he came to me in the middle of November, 1911. I put him on my cane sugar treatment, and he has steadily improved up to the present time, as will be seen by following the dates of the series of tracings. From the middle of March, 1912, till now (July 24th, 1912) he has not had an attack of tachycardia. I saw him on July 21st,



Tracing A.



Tracing B.

and he said he was quite well and had not had an attack. Since March, 1912, he has been working at odd jobs, such as felling trees, hedging, and haymaking from 6 a.m. till 9.30 to 10 p.m. All these are hard work, and yet he has not had an attack. Since November, 1911, he has not taken any drugs, but he has taken the cane sugar treatment the whole time till now, and is still taking it.

In another case there was about a month's treatment between the taking of the two tracings (A and B), and I bring these two sets of tracings before your notice to show how rapidly the cane sugar treatment improves the heart in suitable cases.

DISCUSSION ON THE RÔLE OF CALCIUM SALTS AS THERAPEUTIC AGENTS.

OPENING PAPER.

By Sir JAMES BARR, M.D., LL.D., F.R.S.E.

IT is to me very gratifying to find so much interest taken in the present day in the use of the lime salts, because it is not very many years since I was looked upon as a bit of a crank on the subject, and the students had a pantomimic verse about myself and calcium chloride. Two years ago I took a fairly extensive survey of the subject in an address on the use and abuse of the lime salts in health and disease,[1] and I shall now only devote some attention to a few points in this large subject.

In an interesting paper on Facts and Fancies in Pharmacology,[2] Professor Dixon, F.R.S., has pointed out that calcium taken by the mouth is absorbed to a slight extent and very slowly, and is about as rapidly excreted. It is very fortunate that this is the case, otherwise many people would run the risk of being petrified. However, the amount and rapidity of absorption and excretion vary very much in different individuals in health and disease. I have shown that a much greater quantity is excreted by the kidneys than is generally supposed, and hence, when those organs are diseased, the calcium content of the blood is very apt to rise, especially so as those patients are frequently deluged with milk, a very popular but often not very appropriate diet for invalids.

This retention of lime salts in the system is a strongly contributory cause in the production of cardiac hypertrophy, owing to the contraction of the peripheral arterioles and increased viscosity of the blood. I pointed out the dangers likely to arise from the recent craze for sour milk, owing to the excessive supply of lime. This craze is now disappearing, but some of the expensive and injurious milk foods still have a strong hold, both on the medical and public minds. Properly home-cooked natural food is better than any artificial product on the market. The old Scriptural quotation that milk is good for babes and sucklings, but stronger food for those of riper years, is well worth remembering, especially by teetotalers.

Intestinal stasis and its deleterious effects have recently attracted a good deal of attention, but, strange to say, the medical profession is devoting its efforts to the cure of the mischief, while its prevention is almost entirely neglected. This is what might be expected in a disease where surgical intervention is a prominent feature, but it does not reflect much credit on those who see the affection in its early stages. Metchnikoff and Arbuthnot Lane seem to think that we would get on very well without our large bowel; but as this state of matters is not likely to be attained, in the ordinary process of evolution, during the next million years, those of us who do not hold Lamarckian ideas must try and get the large intestine to functionate regularly without any radical transformation in the animal economy. In the production of intestinal stasis the lime salts play a very important part. No doubt a minute quantity of active calcium ions is essential for the muscular contraction of the intestinal tract; but when you get an excessive amount of lime in the wall of the bowel the muscle contracts, often spasmodically, and the bowel remains contracted without any regular peristaltic action. Just as in the case of the heart, so in the bowel, you must not merely have calcium but also potassium ions for regular rhythmic action.

Dr. O. T. Williams has shown the injurious effects of insoluble calcium soaps on the intestinal mucosa. These soaps form the basis of intestinal sand, which becomes very irritating, and no doubt often leads to ulceration of the mucous membrane. The intestinal flora and their toxins have thus a fair opportunity of setting up inflammatory action in the whole colon and, to a less extent, in the small bowel. One of the worst cases of spasmodic entero-colitis with large quantities of intestinal sand which I have ever seen had been previously under the care of a physician who had kept the patient for six months largely on a milk diet with a liberal allowance of fat. The patient had been placed under this gentleman's care on account of a paper which he had published giving a vivid description of the symptoms of colitis. There was no statement as to how far the symptoms may have been manufactured by the treatment, but at any rate the patient got rapidly well under a more rational diet, and has remained well during the past nine years.

When ulcerative colitis is set up, a vicious circle is soon established so far as the lime salts are concerned. There is not only defective absorption, but also excessive excretion by the bowel, and thus the whole system is depleted of lime, the calcium content of the blood is lessened, and consequently the amount in the urine is small. The patient gets into a miserable condition, partly owing to intestinal toxaemia and partly to deficiency of calcium. The heart's action becomes impaired, the circulation feeble, and extremities cold. There may be

great emaciation from loss of fluid and defective nutrition. The bowel then, in place of being contracted, becomes partially paralysed, and you are apt to have constipation alternating with diarrhœa. In such cases it is absolutely essential to get some lime into the system, and for this purpose I usually prescribe some very active and soluble calcium salts, such as the iodide or glycerophosphate. Of course, the lime may be given subcutaneously, and when this course is adopted it should be largely diluted with normal saline to which about 5 per cent. of glucose has been added. These patients require a considerable quantity of fat, but this should be supplied in an unsaturated form, such as olive oil, cod-liver oil, and butter. Underdone red meat should be freely given.

In atony of the stomach and intestines there is a deficiency of lime—at least, in the walls of these organs—while in hypertrophic stenosis of the pylorus, whether in infants or adults, there is an excessive amount of lime. In advanced cancer of any part of the body there is a diminution in the amount of lime in the blood and tissues, and this diminution is associated with increased alkalinity of the blood.

As I have previously stated, a large quantity of the lime in the blood is excreted by the kidneys. So long as the urine is acid the calcium is usually in solution (except in the form of calcium oxalate) and so causes no local mischief, but when the urine is alkaline there is apt to be a cloudy deposit of mixed phosphates, though this is not likely to cause any mischief unless calcium be the principal base.

In gouty individuals, especially in those who are large meat-eaters, you get an excessive amount of phosphates, and if there also happen to be an excessive quantity of calcium you get numerous small calculi composed almost entirely of the insoluble phosphate of calcium. I recently came across a gentleman who had passed some thousands of such calculi. He was gouty, a free meat-eater, and he unwittingly supplied the lime by drinking large quantities of a Continental water which has acquired an undeserved popularity in the treatment of gout, although it is loaded with lime. I know of another instance in an old gentleman who passed such calculi which more than filled an 8-oz. bottle. In his case the lime was derived from milk. It is often said that the kidneys only excrete the excess of lime in the circulation, and the blood retains its normal quantity, but this is not so; in such cases as those to which I have just referred the excretion may be more rapid than the absorption, the calcium content of the blood falls, and the heart's action may become rapid, feeble, and very irregular from the deficiency in free calcium ions. In these cases I frequently prescribe a mixture of calcium iodide, potassium chloride, tincture of iodine, glycerine, and water.

I shall now show you the amount of lime in the water of Liverpool and of many of the popular Continental spas. After this demonstration I hope you will come to the same conclusion as myself, that people who pay often heavily for Continental waters when they can get much better at home for almost nothing are either fools or suffer from gross ignorance. An amusing *contretemps* often happens at these Continental spas when the physicians tell their patients that they will feel the benefit when they return home, that is, when they have ceased drinking the water, though these physicians do not see the humour of their remarks. I shall not trouble you on this occasion with any further remarks about the lime salts, but I hope I have given you some pabulum for thought, and set loose any plaster-of-Paris which may have hitherto fixed your ideas.

REFERENCES.

[1] BRITISH MEDICAL JOURNAL, September 24th, 1910. [2] Ibid., August 28th, 1909.

DISCUSSION.

Professor R. B. WILD (University of Manchester) called attention to the want of co-ordination between the known pharmacological actions of calcium, its pathological relations, and its therapeutic uses. Disorders of metabolism might result from a disturbance of the calcium balance in either direction, and the effect on heart and skeletal muscle in maintaining vitality and contractility was, like the action on the coagulability of the blood, only found within comparatively narrow limits. The question as to the absorption of calcium salts when given by the mouth was apparently still unsettled, and in some cases the excretion of calcium by the kidney appeared to take place in an irregular cyclical manner. In regard to the clinical use of calcium in the erythematous and urticarial group of skin diseases, good results were found from the administration of the chalk mixture as well as from the soluble salts, and might be due to local sedative action on the alimentary canal. Other cases might be cured by stopping milk and other calcium-rich foods.

Mr. G. R. MINES (Cambridge) said: There has been a tendency to regard experiments concerning the action of chemical substances on living tissues as being necessarily directed immediately towards therapeutic ends. It being shown that, under certain experimental conditions a particular substance can modify the behaviour of some organ, let us say the heart, the physician not unnaturally inquires whether such modification may not similarly be produced in the patient in whom it would be desirable. He is perhaps disappointed to learn that the effect is to be brought about only when the heart is cut out of the body and perfused, or that the active substance in sufficient dose to produce the desired effect on the heart would of necessity paralyse the respiratory centre. He is inclined to say that such experiments are "of no practical value."

The real object of these physiological experiments is not to determine whether or how the chemical substances may be employed as therapeutic agents, but to learn, by the study of their action, something about the mechanism of the tissues.

In order to discover by what sort of mechanism a tissue exerts those functions which constitute its life, a fruitful method is the investigation of the relations of its activity to its environment. Now this means of necessity, in most cases, a more or less extensive departure from its normal environment. Obviously it is of the first importance to know as precisely as possible what the environment is. What is called "the normal environment of tissues" is of immense complexity, and at present we are unable exactly to define it. From a physiological point of view the composition of blood or lymph is only very roughly and imperfectly known.

Normal conditions are at present unknown conditions. The position has often been maintained—and there are some who would defend it still—that in all physiological experiments it is desirable to keep the tissues under investigation "as nearly normal as possible." In many cases this position can be justified, and particularly in those experiments where the object is to determine directly the therapeutic action of chemical substances. But when the object is the investigation of function of some particular part of the body mechanism, the method of injection into the veins of an intact animal is far too crude to yield the kind of information that is sought. It is necessary to insist a little on this because the opposite view has been strongly advocated. Blake, for instance, asserted that the proper way to investigate the action of salts was to inject them into the veins of an animal. Thus, he said, one could find the action on respiration, on the muscles, on the kidneys, on the heart, and so forth. He poured contempt on the methods which Ringer adopted; it seemed to him fantastic and absurd to set about the investigation of the physiological action of salts by "pumping them through the heart of a dead frog." Yet it becomes more and more clear in the light of recent work that there are all sorts of difficulties in the elucidation of the chain of events connecting the injection of some chemical substance into the blood and the appearance of some characteristic effect in an organ.

For example, Elliott has lately shown that the administration of drugs may produce effects on plain muscle in the body by the following very indirect route: The drug stimulates a nervous centre in the medulla; nerve impulses are sent through the splanchnics to the suprarenal bodies; the suprarenals respond by discharging adrenalin into the blood, and the adrenalin acts in turn on the myoneural substance of the plain muscle.

Apart from complications of this type, it must be remembered that the injected substance may very possibly undergo some chemical change in the blood or in passing through some organ before it reaches that which is the

object of investigation. The chemical nature of the substance actually reaching the organ is therefore uncertain, while the concentration in which it is presented must be vague. For some purposes, as I have said, this type of experiment is valuable, but as a means of investigating local mechanisms it is hopeless. It would be almost as profitable to attempt to investigate the psychology of the Home Secretary by sending him letters through the post, couched in various terms, and scrutinizing the official replies.

As the immense complexity of the interaction of nervous and chemical co-ordinating mechanisms in the body is revealed, the necessity for the simplication of conditions when any special part is under study becomes more and more obvious. Not only do we cease to apologize for artificial conditions of experiment, we demand them. It is not expected that such experiments will provide short cuts to discoveries in therapeutics (though incidentally they may give useful suggestions), but there can be little doubt that on the attainment of their objects they will come to form part of the foundations of scientific medicine. It is significant that the physiologist to whose admirable researches the recognition of the importance of inorganic salts for the activity of living tissues is chiefly due—namely, Sidney Ringer—was himself a renowned clinician.

Of the various conditions under which it may be helpful to study the behaviour of tissues, naturally the most interesting will be those under which the tissues continue to manifest the functions which characterize their life.

Muscles and nerves give "signs of life" which we are able to study particularly readily, and Ringer showed how the conditions under which these tissues could continue to exhibit their "signs of life" could be reduced to their simplest terms. He worked chiefly on isolated organs, recognizing that a knowledge of the mechanism of its parts is essential to an understanding of the body as a whole. He pumped fluids through the heart of a dead frog! Ringer's triumph lay in the fact that though the frog was dead its heart was alive, and living under conditions more precisely defined than ever before. It is possible to adjust the conditions so that the tissues shall behave apparently as they do in the body. By modifying the conditions we may make them behave as they never could in the body, and these observations are as helpful as those, for it is by finding out *what a tissue can do* that we may hope to learn what it is.

Let us review very briefly some of the results of experiments on isolated tissues with respect to the importance of calcium salts. I will confine myself to skeletal muscle, neuro-muscular synapses, and heart muscle. We recognize that these tissues can continue to exhibit their characteristic functions when their chemical environment is simplified to a solution of sodium (potassium) and calcium chlorides in distilled water. For the tissues of the frog a concentration of sodium chloride 0.125 mg., or about 0.7 per cent., is suitable, while of calcium and potassium about 0.0025 mg., or, very roughly, 0.02 per cent. of each, is sufficient.

In this simple Ringer's solution the excitable condition of various tissues may be preserved for a considerable time. The heart will beat vigorously for many hours when perfused with such a solution, and, as Kuliabko has shown so strikingly, mammalian hearts may be resuscitated several days after the death of the animals. With the tissues permeated with Ringer's solution excitation can pass from nerve to muscle; and skeletal muscle, if putrefaction be avoided, will remain capable of responding to an electric shock with a contraction even as long as three weeks after removal from the body.

What are the effects of omitting calcium from the solution?

We will take the case of skeletal muscle first. Placed in a solution free from calcium, the sartorius muscle of the frog within a few minutes, often within a few seconds, exhibits lively spontaneous movements, which may continue at intervals for many hours.

It is very remarkable how quickly the effect of removal of the trace of calcium from the surrounding fluid appears in the muscle. The change in the condition of the muscle fibres which leads to these spontaneous contractions may be detected also by a change in the excitability of the tissue towards galvanic currents. Though there is little or no change in the excitability of the muscle towards induction shocks, there is a very marked increase in excitability towards electric currents of long duration. Moreover, any change in the surrounding fluid which tends to increase the spontaneous movements increases also the excitability towards stimuli of the type named; any change abolishing the spontaneous movements depresses the excitability towards galvanic currents.

The spontaneous movements are stopped, and the excitability reduced to normal by a small concentration of calcium. It is interesting to note that the spontaneous movements are due to rhythmic excitations originating in the muscle fibres themselves, and are independent of nerves or nerve endings. The abnormal condition of the muscle may be corrected not only by calcium, but also by strontium or magnesium salts.

It is necessary, of course, in experiments of the type we are discussing to make sure that the experimental solutions are really brought into intimate contact with the tissues. In the case of a thin muscle like the sartorious of a frog, the condition is realized very well by simple immersion; when a thicker muscle is taken, a much longer time is needed for the attainment of equilibrium between tissue and surrounding fluid. Thus if the frog's gastrocnemius is immersed in pure sodium chloride solution, the twitches do not start for about an hour. Much more prompt results are obtained by perfusing the experimental solutions through the blood vessels of the tissues. By this method it becomes possible to explore certain other relations of calcium. It is found that if the muscle is well washed out with a solution of sodium chloride, or of sodium chloride *plus* a little potassium chloride, after a time the muscle no longer contracts in response to the stimulation of its motor nerve, although it is readily made to contract when the stimulus is applied directly to the muscle. The block, like that produced by curare, lies not in the nerve, but in the myo-neural junction. A small concentration of calcium will restore the functional connexion between the nerve and the muscle; the calcium may be replaced by strontium or, less perfectly, by barium; but in this case magnesium is absolutely useless. Quite similarly in the heart, the connexion between the inhibitory nerves and the muscle requires the presence of calcium or of one of its close chemical allies, strontium or barium, for its completion. The heart perfused with a solution containing neither of these will continue to beat feebly for some time; but while in this condition it cannot be stopped by stimula- of the vagus nerve. The addition of a little calcium or strontium chloride to the perfusion fluid enables the vagus to exert its usual effect on the heart.

Perhaps the most striking instance of the importance of calcium is in relation to the activity of the heart muscle itself. It was shown by the classical experiments of Ringer that, if perfused with a solution free from calcium, the heart of the frog becomes feeble in its beats, tending to stop in diastole. Conversely, an excess of calcium makes the beats systolic in character and, if too great, stops the heart in systole. Here, again, the calcium may be replaced very satisfactorily by strontium, and to a certain extent by barium, while magnesium acts in a totally different fashion. It is interesting to note that the importance of calcium in relation to the activity of heart muscle is by no means confined to the hearts of amphibia. Similar relations are found to hold in mammalian hearts, in the hearts of fishes, and of molluscs.

It has been said that calcium is "the stimulus for the heart beat." Such a statement is, on reflection, meaningless. The statement is based solely on the fact that the heart will not beat in the absence of calcium. But the heart will not beat in the absence of water or if its temperature is too low. It would be equally reasonable to say that water or warmth is "the stimulus" for the heart beats. (Once the conception of irritability has been grasped, there is no objection felt to the use of the expression "spontaneous" rhythm.)

What we can say fairly is that the presence of the Ca ion, or of ions chemically akin to it, is one of the chemical conditions necessary for the manifestation of the normal activity of heart muscle.

I have recently made experiments in which the electrical variation, as recorded by the string-galvanometer, is studied simultaneously with the mechanical behaviour of the perfused frog's heart. The leading-off electrodes are placed—one on the sinus, and the other on

the apex of the heart. (The electrograms thus obtained, it is curious to note, often resemble the human electrocardiogram to an extraordinary degree.) When the perfusion fluid is changed to one containing no calcium, it often happens that the contractions of the heart are reduced in amplitude to a very great extent, while the electric variations are very little affected. We have many reasons for believing that the excitation process in muscle can be dissociated from the contractile process, and it seems clear that the electric variation is a direct index of the excitation process, while the contraction is a sequence, usual but not quite inevitable. Just as in nerve excitation occurs (but no contraction), so in muscle excitation occurs; if the contractile mechanism is in order, excitation is followed by contraction.

The experiments just described rather suggest that calcium is of pecular importance for the contractile process in heart muscle. It is, however, also of importance for the excitation process; the frequency of the excitations is affected by removal of calcium, and later there may be interruption of the sequence of auriculo-ventricular excitations.

With regard to the question as to how Ca exerts its actions, one thing is quite certain—namely, that it is not always in the same way.

We know that any salt of calcium which is soluble and which, according to the physical chemists, yields Ca ions, is efficient. We say, therefore, that the calcium ion is responsible for the various effects we have to consider.

Now a particular class of effect of ions on colloidal systems, particularly colloidal systems involving membranous structures such as we have to deal with in the living tissues, is produced by ions simply by virtue of the electric charges and mobilities of the ions, irrespective of their chemical nature. Where Mg, like Sr, can produce the same physiological effect as Ca, there are strong reasons for believing that we have to deal with an "ionic" effect.

From the physico-chemical aspect, the Mg ion is as much like the Ca ion as is the Sr ion. They all carry the same electric charge and move with much the same velocity. With artificial systems of membranes it is easy to demonstrate electrical and imbibition effects in which these ions all act closely alike. As we have seen, in the case of skeletal muscle these ions have closely similar effects on excitability; they also behave very similarly in their antagonism to the action of potassium—an antagonism which can in fact be shown quite readily in an artificial membrane schema.

But, in addition to such "ionic" effects, and not infrequently masking them, there are more specific relations, due to the chemical combination of ions with the materials of which the tissues are composed. In the case of calcium the latter type of action is of predominant importance. In relation to the contraction of heart muscle and in the transmission of excitation from nerve to muscle, Mg is quite unable to take the place of Ca. This inability is not due to any injurious effect of Mg on the tissues, for Mg in the molecular concentrations with which we are concerned in no way prevents the restoration of function by Ca and its allies Sr and Ba.

Evidently in these cases we have not to do with a simple ionic effect. We know that chemically there are well-marked differences between certain of the compounds of magnesium and the corresponding compounds of the metals of the alkaline earths. Purely as an illustration, we may contrast the sulphates of these elements. As regards solubility and other physical features, the sulphate of Mg is strikingly different from the sulphates of Ca, Sr, and Ba. The same is true of the citrates of these elements, and may very well be true of the compounds which they form with the proteins of the tissues. It may be that an ion-protein compound with particular physical properties is a necessary part of the particular mechanisms we are now discussing. As Professor Moore has remarked, the reversibility of such a compound will be a matter of importance. These ideas are strongly supported by comparative observations of the effects of Ca, Sr, and Ba. Just as we trace progressive decrease of solubility in the series of sulphates ($BaSO_4$ is one of the most insoluble substances known, $SrSO_4$ is intermediate between $BaSO_4$ and $CaSO_4$), so we see a progressive change in the physiological action of the ions Ca, Sr, and Ba. This is shown in several instances, best in the heart. Adding equivalent concentrations of Ca, Sr, and Ba solutions in turn to the perfusion fluid, all produce increased tone and lengthening of the systole, but the effect of Sr is markedly greater than that of Ca, while Ba leads quickly to permanent arrest in extreme tone. It is as though Ba formed a compound similar in type to those of Sr and Ca, but almost irreversible. The study of electrolyte action provides much information which aids in the analysis of the functions of tissues. Above all, it is necessary to recognize that the field is very wide and that any attempt to attribute all the manifold effects of salts on living tissues to some single factor can only hinder progress.

Recognition of the interplay of various factors helps to clear up many points which otherwise are inexplicable. Let me conclude with one instance. Several speakers this morning have referred to the curious fact that though the presence of calcium is necessary for the clotting of blood, too much calcium hinders clotting, and there is thus an optimal concentration. The explanation seems simple enough. A certain small amount of calcium is needed for certain preliminary changes necessary for clotting. So far as these are concerned the amount of calcium is immaterial provided it reaches a certain minimum. But, as is well known, the clotting of blood is hindered by any large concentration of a neutral salt; it is, in fact, a common method of preventing coagulation in blood pressure experiments. And a large concentration of a calcium salt is as effective as a large concentration of a magnesium salt in hindering the conversion of fibrinogen into fibrin.

Dr. W. BLAIR BELL (Liverpool) said: I had no thought of being able to be present or to take part in this discussion. I have therefore prepared nothing, and must rely on the few notes I have just made concerning what has been said by the previous speakers. I have been extremely pleased at the pre-eminent position that has been assigned to Sidney Ringer (and with him I would associate the name of Dudley Buxton) as the real discoverer of the great physiological importance of the calcium salts. I am more proud of having been the first in recent years to recall attention to his work, than I am of anything I have myself done in regard to the calcium metabolism. For although it was not until I had been working at the subject for some time that I came across his papers, when I did so they inspired me to further efforts; and they still remain in my opinion the finest and most original contributions that have ever been made to the subject under discussion. Sidney Ringer was a clinical physician as well as a laboratory worker; and within earshot as we are of the surgical ward in which I work, I think it may not be out of place to say that I hope pure laboratory workers will give the greatest possible encouragement to clinicians to work with them. I feel convinced that in this way alone the best results can be obtained. As you are probably all aware, I was led to take an interest in the calcium metabolism by the discovery of the very important part it plays in the physiological and pathological processes of the female genital organs. But as these specialized processes are largely dependent on more general conditions, I was led far afield in an endeavour to complete the picture. As I have just published a paper dealing with calcium therapy in gynaecological and obstetrical disorders,[1] I must refer you to that for the special part in which I am particularly interested, and confine my attention to-day to the more general, and to you more interesting, aspect of the subject.

I think we must divide the consideration of the metabolism of the calcium salts into three periods in the life-history of the individual.

(1) The period of growth, when calcium is being collected for the building-up of the bones and tissues; (2) the period of maturity, when a balance is maintained between storage and excretion; and (3) the period of old age, when elimination becomes inefficient, or decreased oxidation of the tissues allows the deposition of lime salts.

It is in the middle period that we have the largest amount for excretion at our disposal; and women can supply their offspring with what it requires in the womb, and subsequently from her milk. Such claims which may be thrown upon a woman show us how active must be her metabolism, and how well adjusted it is. This brings us

[1] *Liverpool Medico-Chirurgical Journal*, July, 1912.

to the question of the regulating factors of the calcium metabolism. And when we offer to consider regulating factors in the living subject, it should be understood that behind these again are less understood primary factors controlling all that is obvious to us, and that the best we can do is to go back step by step. I believe, then, that immediately behind the activities of the calcium metabolism lie the secretions of the ductless glands, which, as I have just said, are subject themselves to other controls.

But this recognition of the position of the ductless glands is, I believe, of vast clinical importance. I can now only give one example: Osteomalacia, which has till recently been treated by the removal of ovaries, is now successfully treated by the injection of adrenalin or of infundibulin (post-pituitary extract)—a preparation I have had the satisfaction of helping to establish in clinical practice, although the credit for the discovery of its physiological importance rests with Schäfer and Oliver.

Calcium is taken into the system by absorption from the alimentary tract; and in regard to this I differ from what has been said by Professor Dixon and Professor Wild, if I have correctly understood them to say that calcium salts are with difficulty absorbed from the intestinal tract. What I believe represents the truth is this: The healthy individual can control, through his ductless glands, the quantity delivered to his tissues, and he can readily excrete any excess. My views concerning the influence of calcium salts on the clotting of blood coincide with those so admirably demonstrated by Addis—that is to say, there is, as I stated long ago, an "optimum amount." Less than this causes delay in coagulation, but moderate excess neither increases nor retards the rate of clotting. Great excess, which cannot be produced by oral administration, but only by intravenous, appears to retard the coagulability of the blood. It is interesting that this conclusion was arrived at by Sir Almroth Wright on apparently wrong scientific data. But it is not the first time that a correct hypothesis has been demonstrated incorrectly. I believe that the direct control of haemorrhage alluded to by Professor Wild is due largely to vasomotor constriction, although if the calcium index of the blood be low the establishment of the normal allows of normal coagulation.

I now come to a most interesting question which has entirely escaped notice in this discussion. Once more I am indebted to Ringer for the original suggestion to which I have given, and I believe correctly, clinical significance. Ringer, in a most interesting little communication, showed that the laminaria "tent," when immersed in distilled water, swelled at a certain rapid rate, and that the rapidity of swelling—that is, of the imbibition of fluid—could certainly be checked by the addition of calcium salts to the water. It is long since I read the paper, but, speaking from memory, I believe he showed that there was some direct relationship between the quantity of lime in the solution and the rate of imbibition. Without wishing to infer that the cells of the dried laminaria tent are absolutely comparable in this respect to those of the living animal, I would like to bring before you the suggestion, which I have several times referred to in other communications, that the beneficial effect often produced by the administration of calcium salts on urticaria, on chilblains, and on local oedemas is due to the fact that the tissue cells cannot readily imbibe fluid rich in calcium salts. The view of Sir Almroth Wright, who, I believe, first suggested the use of calcium salts in such conditions, was that the coagulability of the blood was increased, and leakage from the vessels, therefore, did not occur so readily. This interpretation of his clinical observation is, I think, incorrect, especially in view of our present knowledge regarding the slight or negative effect of oral administration on the coagulability of blood not deficient in the optimum amount of calcium. The view I have expressed regarding imbibition, which I owe to Ringer's experiment, I regard as more likely.

I have not the time to go in any detail into the many other interesting questions of pathology in which the calcium salts play an important part. Many references may be found in communications of other writers, as well as in my own. And many interesting suggestions are to be met with. You have had an opportunity of seeing the valuable demonstration of Sir James Barr of the injurious properties of many of the much vaunted waters for the treatment of stone in the urinary tract. And I think he is much to be congratulated on so forcibly calling attention to this matter.

With regard to the general principles underlying the pathological deposition of the lime salts in the tissues, we must all agree with the main chemical proposition suggested by Professor Benjamin Moore that deficient oxidation is present. But I would emphasize more especially in clinical terms the fact that the tissues are diseased (for example, arteritis) or degenerated (as in fibromyomata). Yet in regard to fibromyomata, it has often struck me as curious that "egg-shell" calcification should occur, in which calcification occurs at the periphery where the circulation is usually good, while the interior of the fibromyoma is not calcified. Many fibromyomata uteri, of course, calcify in the interior.

There is only one more heading under which I wish to make a few remarks—in particular on what has been said by Professor Wild. I do not believe, as he does, that one can get such good effects by the oral administration of insoluble compounds of calcium as with the soluble salts. For various reasons I think there is no comparison. I cannot realize how chalk can be absorbed so well in calcium lactate. So convinced have I become of the importance of administering soluble salts that I have the calcium lactate made in solution. I believe also in giving large doses—gr. xxx or ʒj of calcium lactate every night for a week on an empty stomach, and then on alternate nights. I have found, too, that these large doses do not have a constipating effect, whereas, of course, chalk is extremely constipating. Anything that increases unnecessarily the too prevalent condition of constipation in woman is much to be deprecated.

Subsequently Dr. BLAIR BELL, in answer to the suggestion of Professor Dixon that the slightly aperient effect mentioned of large doses of calcium lactate would be due to the action of lactic acid, and was similar to the effect produced by sodium lactate, expressed dissent from this view. The action was, he thought, due to increased peristalsis, and it only occurred about twelve hours after taking the drug. The presumption of Professor Dixon that because sodium lactate was an aperient (owing to the effect of lactic acid) so also was calcium lactate, was not borne out by analogy. Sodium sulphate was an aperient, but calcium sulphate certainly was not. The suggestion of Professor Wild that the calcium carbonate and calcium lactate were both completely converted into calcium chloride in the stomach required careful consideration. But the strength of free HCl in the stomach was usually about 0.2 per cent., and this was insufficient for the purpose suggested by Professor Wild. However, such differences of opinion and interpretation of facts only showed how much work still remained to be done on the subject.

Dr. F. RANSOM (Cambridge) said: Amongst the many interesting facts which are associated with the metabolism of calcium, perhaps none is more striking than the enormous loss of this substance which occurs in numerous cases of diabetes mellitus. I have lately had an opportunity of examining the urine of two diabetics, especially in reference to the excretion of calcium, and I think that some observations on the results obtained will fit well into the present discussion.

So far as concerns the percentage of sugar contained in the urine of these two individuals, C. and W., there was no great difference. C.'s urine contained on an average from 5 per cent. to 6 per cent., W.'s from 6 per cent. to 7 per cent.; but, whereas C. usually passed about 1,500 c.cm. of urine in twenty-four hours, the quantity excreted by W. ranged from 2,500 c.cm. to 3,000 c.cm. Both were kept on a practically identical diet and both were in middle life. C. was stout and well nourished, W. extremely thin. From the difference in the total amount of sugar passed daily you will see at once that whilst W. had to a very large extent lost the capacity for utilizing carbohydrates, C. still retained considerable powers in this respect. Further—and this is the point to which I desire to draw your particular attention—W. was losing daily in his urine quite a large quantity of calcium, whereas C. did not excrete more than may be found with non-diabetics; 200 c.cm. of W.'s urine contained on a rough average about 0.080 gram

of CaO, which means that he was losing about 1.2 gram of CaO daily. On the other hand, C.'s urine contained on an average only about 0.0170 gram in 200 c.cm., which puts his loss of Ca for twenty-four hours at 0.12 gram, just one-tenth of what W. was losing. I may say that the observations were continued through a period of from seven to eight weeks, and that in each case some forty or more calcium estimations were made.

Opinion in general is agreed that this excess of calcium in diabetic urine is conditioned by the excretion of β-oxybutyric acid, and in accordance herewith I found this body in W.'s urine, but not in C.'s. The experiments of Minkowski and others have shown that β-oxybutyric acid may very well be a stage in the normal metabolism of fat, though it does not appear in the urine because it is completely oxidized—burnt off. Oxybutyric acid appears, however, in the urine of normal persons if carbohydrates are omitted from the diet, as also in prolonged hunger. For this and other reasons it has been concluded that if the quantity of carbohydrate burnt in the body sinks below a certain amount β-oxybutyric acid will be excreted, which is much the same as saying that for the satisfactory oxidation of fats the oxidation of a certain amount of carbohydrates is necessary. This view receives confirmation from the varying amounts of Ca excreted respectively by C. and W., for it is obvious that, as under an identical diet W. excreted more sugar than C., the latter had retained a very much greater power of burning off the carbohydrates than had W. As a result of this W. had to get rid in some way of the oxybutyric acid which he could not burn, and so his store of calcium was attacked.

Now, such a loss of calcium may be a very serious affair. The experiments of Ringer, Locke, Loew, Chiari, Januschke, Loeb, and many others have demonstrated the important part played by Ca. Calcium is necessary for the building up of the protein complexes in cell nuclei, and all calcium precipitating substances are cell poisons. The grey nervous substance is richer in cells and much richer in calcium than the white substance. Moreover, to come nearer to diabetes, Schrank has shown that within certain limits $CaCl_2$ is able to hinder the development of adrenalin glycosuria. In this direction I have lately made some experiments the results of which go to confirm Schrank's observations. The partial withdrawal of calcium has, as H. Meyer points out, the effect of causing an increased excitability of the autonom and sympathetic systems, so that, for instance, atropine acts longer and more powerfully upon the heart vagus than under normal conditions. The remarkable experiments of Chiari and Januschke have demonstrated the power which calcium possesses of limiting exudation into the pleura, such as follows the action of diphtheria toxin, and of diminishing or preventing the inflammation which oil of mustard sets up. Hamburger and Hekma have shown that calcium greatly favours phagocytosis.

In view of all these facts it seems highly probable that the intercurrent illnesses to which diabetics are notoriously liable may be, partly at any rate, due to the withdrawal of calcium. It is, indeed, conceivable that in some cases loss of calcium may be a factor in the etiology of diabetes. It should be a matter of consideration for the physician how he may best check the drain of calcium by putting other alkalis at the disposal of the oxybutyric acid, or possibly by giving calcium to replace the loss. The latter point is especially noteworthy, since it has been asserted that the administration of calcium materially improves the condition of diabetics. In this connexion it may be noted that Rey asserts that with a single subcutaneous injection of calcium one can cause a material rise in the calcium content of the blood lasting over several days.

COD-LIVER OIL AND ITS ACTION IN PHTHISIS.

By OWEN T. WILLIAMS, M.D.Lond., B.Sc., M.R.C.P.,
Honorary Assistant Physician, Royal Infirmary, and Lecturer in Pharmacology, University of Liverpool.

IN the midst of the discussions of the modern methods of treating phthisis it is not without profit to review and reconsider some of the methods of treatment hitherto used. The object of this contribution is to show that cod-liver oil, which has been so much used, must still be considered a most valuable factor in the treatment of the disease. I have elsewhere shown[1] that the oil contains fatty bodies which have a markedly beneficial action on metabolism in phthisis, and that possibly it may have, even in the body, a direct action on the tubercle bacillus.

With Dr. Forsyth I demonstrated that the administration of cod-liver oil very definitely increased not only the total absorption of fat, as it naturally would do, it being so easily assimilable, but also that it increased the percentage absorbtion of all fat taken. Further, it very markedly influences the retention of nitrogen, a fact which is explainable by the findings of McLean and myself,[2] that the so-called tissue fats are really lipoids—that is, fat holding in chemical combination nitrogen and phosphorus. Cod-liver oil *in vitro* dissolves the fatty envelope which surrounds the tubercle bacillus. These effects I have attributed to the large amount of unsaturated fat in the oil, and in previous communications I have given reasons for believing that these unsaturated fats can reach the tissues as such, and so exert their beneficial action.

In relation to this, I should like to mention the work which, with Dr. Wilson,[3] I did on the blood in diabetes, in which it was shown that in this disease the unsaturated fats were used up, and that when lipaemia occurred, as it did in ten out of sixteen cases, the blood was loaded with an excess of the saturated fats. In view of the above findings, it appears to be at least a remarkable coincidence that in diabetes, where the unsaturated fats are so much diminished, phthisis is so common.

Dr. Mildred Powell has recently carried out some experiments which tend to show that unsaturated fatty acids inhibit the growth of the tubercle bacillus. We observed very definite inhibition of the growth of the organism on media containing amounts of unsaturated fatty acids, such as would occur in the blood. On media containing the same percentages of saturated fatty acids growth occurred. The inhibition was also present with the sodium soaps of the unsaturated acids, but not with their glycerides. From the fact that growth occurred normally in the presence of the soaps, it would appear that the inhibition is due to the nature of fatty acids, and not to acidity alone.

These observations tend to show that there is justification for the great faith held in cod-liver oil, which, with the introduction of more recent methods, seems to be declining.

Dr. C. J. B. Williams was, perhaps, the keenest advocate of cod-liver oil, but for many years following his time all authorities were agreed as to its supreme value. The reviews of fifty years ago all state that they are convinced of its value, and while deprecating some of the marvellous effects attributed to it, predict that it will always be considered of the greatest aid in treatment.

In the early part of the last century, when Laënnec showed the true nature of phthisis, the disease was looked upon as incurable, lasting only a short period; then comes a distinct note of hope as the result of the administration of oil.

The numerous cases recorded at that time show conclusively that the disease could be arrested for many years. Of the many writings of that period, I have chosen a series of twelve papers written by Dr. C. J. B. Williams in 1867 and 1868, to compare the results with those of to-day. I have chosen these because they contain an excellent series of closely analysed statistics, which may *roughly* be compared with our statistics of to-day.

Medical statistics containing no constants, and so many variables, can under the best conditions rarely lend themselves to biometrical consideration. In two series of statistics so far apart as 1868 and 1910 it is useless to consider any real correlation; nevertheless, I believe that the figures as we find them can teach us a great deal. In the *Medico-Chirurgical Transactions* of 1844 Williams considers the duration of life of 1,000 of his cases, of whom 802 were alive when last heard of, the average duration of life being already more than eight years. Fagge criticizes them thus: "Among these cases, however, none were included which had not been at least one year under observation; and this restriction, besides keeping out of the list all rapidly fatal cases, doubtless weeded out most who failed to improve for a time under

the treatment recommended, and therefore ceased to attend." These are not the only objections which might be raised. That they are phthisis and no other disease is easily seen on reading the papers, the only proof lacking being the demonstration of the tubercle bacilli.

For comparison I give the statistics from Dr. Bardswell's work on *The Expectation of Life of the Consumptives after Sanatorium Treatment*—the result of ten years' work at King Edward VII's and other sanatoriums. In this table Bardswell has adopted the German method of including all cases together, thus making one series comparable with the other. Comparing these statistics, we find in the older series: Cases less than a year under observation excluded, which no doubt makes the cases those of the more chronic type; on the other hand, in the recent series those dying in the sanatorium are not included. The great point of distinction is, in my opinion, to be found in the nature of the cases.

In the sanatorium statistics 25.6 per cent. of the cases are incipient (Trudeau's classification), showing: Slight or no constitutional symptoms; slight or no elevation of temperature or acceleration of pulse at any time during the twenty-four hours; expectoration small in amount or absent; tubercle bacilli present or absent; slight infiltration limited to the apex of one or both lungs or a part of one lobe; no tuberculous complications.

Of the moderately advanced cases, 39.4 per cent., that is: No marked impairment of function, local or constitutional; localized consolidation moderate in extent with slight or no evidence of cavity formation; no serious complications. Therefore the remainder, only 35 per cent. of the cases, were advanced in a degree comparable to the cases published by Williams.

The final results are only quoted to compare in the only way possible the treatment fifty years ago and to-day. It is a comparison which of necessity must be very rough. In the series of cases treated by Williams most of the cases were diagnosed in the third stage, and were, therefore, most of them chronic cases. The statistics are vitiated as shown by Fagge.

With all the advances since made in the recognition of early cases, the immeasurable assistance rendered by the discovery of the tubercle bacillus, the knowledge of pathology and immunity, and the help of *x* rays, we should expect some striking results in the treatment of the disease, but these statistics do not show that advance. We have, on the one hand, a series of 500 cases, which, for the sake of argument, may be called chronic, and, on the other hand, a series of cases of which 65 per cent. are very early as compared with the former (25 per cent. incipient, some of them with no tubercle bacilli demonstrable).

I believe that a comparison of the results, rough though it may be, is sufficient to justify the statement that treatment has in no sense progressed with our knowledge of the disease. For comparison only the final results are given, as little can be expected from a closer comparison of two series of cases collected under such different conditions.

Bardswell's Cases (1910).

234 cases (19 who died in sanatorium not included).
Of 15 cases, four to five years after, 52 per cent. were well or alive.
Of 34 cases, five to six years after, 82 per cent. were well or alive.
Of 55 cases, six to seven years after, 58 per cent. were well or alive.
Of 53 cases, seven to eight years after, 49 per cent. were well or alive.
Of 58 cases, eight to nine years after, 34.5 per cent. were well or alive.

C. J. B. Williams's Cases (1868).

500 private cases (all dying in first year not included).
Five years and upwards living, 66.6 per cent.
Ten years and upwards living, 31.2 per cent.

The following statements quoted from C. J. B. Williams's Lumleian Lectures, delivered in 1862, show that even fifty years ago there were some who appreciated to the fullest extent the hygienic treatment of the disease:

> The more fully we recognize the relations of the disease to the general economy, and the less it is treated as a local affection, the more likely are we to progress. The proper treatment of phthisis seems to be that which is calculated to improve the nutrition, to elevate the condition of health. For the fulfilment of this object we must have an eye to the influence of food, clothing, and air, bodily and mental discipline.

> "Pure air, nutritive digestible food, regular exercise, and well-arranged habits of life—these and their correlative hygienic measures stand in the first place because they are equal or superior in power to any form of medicine, properly so called, in preventing the development of the disease."

Some six years later—that is, in 1868—Dr. C. J. B. Williams also said:

> On making a retrospect of forty years in the treatment of pulmonary consumption, I can trace a remarkable improvement in its success as judged by results . . . and I have no hesitation in stating my conviction that this agent [cod-liver oil] has done more for the consumptives than all other means put together.

In the last half of last century almost every writer of note testifies to the value of the oil, and Fagge states:

> Now that it is taken by almost all consumptives one is apt to underrate its real importance.

Since the first chemical researches on cod-liver oil by Wurzer in 1822, many elaborate investigations have been made with a view to determining the active principle or principles contained in it. No fewer than fifty-one compounds have been found at different times occurring in the oil. Considering the fact that from 95 to 98 per cent. of the oil consists of fatty acid compounds, the other fifty or so are each present in the smallest quantities, and it may now be said that many of them are decomposition products, and some hypothetical. The efficiency of the oil has been attributed to the almost infinitesimal quantities of iodine or phosphorus, or to the peculiar bodies to which fanciful names have been given. An important difference between cod-liver oil and drugs which act in virtue of an active principle lies in the fact that cod-liver oil is in the nature of a food, and its activity is in a different sphere to that of drugs ordinarily so called.

The active principle of a drug enters into a combination with the cell in such a manner that it is loosely held, and can be recovered from the cell. Its activity results by an alteration of the chemical or physico-chemical reactions of the cell without yielding energy to it. Cod-liver oil, on the other hand, enters into direct and permanent combination with the cell, yielding energy to it, and thereby altering the whole of the cell's relations by becoming an integral part of the cell protoplasm. The activity of the oil is, I believe, to be found in those constituents which can perform this rôle, and these are the fats themselves. In recent years this has been perceived in a somewhat hazy manner, and the value of the oil attributed to the fact that the oil is more readily absorbed and assimilated than other fats, and this fact has been lost sight of in consideration of its therapeutic effect.

Recent work on lipoids has taught us that the various forms of fat play an important part in the animal economy and particularly in disease, and it has now to be realized that a recognition of two distinct classes of fatty acid, as regards their physiological action, is needed. The compounds of saturated and unsaturated fatty acids must be considered as playing two distinct parts in metabolic processes.

Loathes has suggested that one of the functions of the liver is the preparation of fatty acids of a high degree of unsaturation for the further processes of metabolism. Recognizing these points, it can be readily understood that cod-liver oil containing fatty acids whose degree of unsaturation is equal to those produced by the liver in its normal action, is a body which can be expected to have a marked action on metabolism. That it has that action is shown in the work mentioned above.

The phosphatides found by McLean and myself in the tissue fats were not found by Mr. G. E. Branch and myself in cod-liver oil, the inference being that they are split up (if they occur in the cod liver itself) and the fats only yielded to the oil in the process of extraction. The tissue fats extracted immediately after death by alcohol and ether before autolysis could occur yielded saponification values higher than those of ordinary neutral fats, this high value being due to the phosphoric acid contained in them.

Five samples of cod-liver oil made by different well-known firms gave saponification values less than those of neutral fats: Sample A, 186.7; Sample B, 181.9; Sample C, 187.9; Sample D, 186.9; Sample E, 187.5. These figures approximate those of the neutral fats, but are in each instance lower.

Direct estimation of phosphorus also leads to the same conclusion—the absence of any but the smallest amount of phosphatides, even if the phosphorus be present in that condition. Carles found no phosphorus in freshly-prepared neutral oil, but in oils containing free acid traces were found. He similarly found that iodine was only present in those oils where decomposition of the liver had occurred. These facts, which we have confirmed, further support the contention that the activity of the oil does not depend on the presence of iodine or phosphorus.

The oils in use, differing in their methods of preparation, contain different quantities of fatty acids, and also different amounts of phosphorus and iodine.

The five samples examined by us gave the following amounts of free fatty acids: A, 0.5 per cent.; B, 0.3 per cent.; C, 0.5 per cent.; D, 8.1 per cent.; E, 0.1 per cent.

The sample E was an oil obtained by extracting the liver in an atmosphere of carbon dioxide to prevent oxidation. Not only does it contain a very small amount of free fatty acid, showing the apparent freedom from decomposition, but it contains, as will be shown, the greatest amount of the fatty acids upon which the activity of the oil depends.

A consideration of the methods of preparation of the oils we examined leads us to believe (thus confirming Carles's work) that the presence of the small quantities of iodine, phosphorus, and various other bodies in cod-liver oil is due to decomposition. From our observations, both in the laboratory and clinically, where we have investigated the effect of the oil on metabolism, I believe that the therapeutic effect of the oil is due to the amount of unsaturated fatty acid it contains, and not to its impurities. We have therefore compared various oils from this point of view, and investigated the nature of these unsaturated bodies in the oil.

The five samples gave the following iodine *values*, representing the amount of unsaturated acids in them: A, 125.1; B, 120.7; C, 126.2; D, 118.4; E, 161.7.

It will thus be seen that sample E (obtained, as before mentioned, by immediate extraction in an atmosphere of CO_2 to prevent oxidation) contains by far the most unsaturated acid.

The unsaturated acids were separated from sample A. These gain an iodine value of 171.1. This would tend to show that in sample E (I value 161.7) the greater amount of the fatty acids present are unsaturated to an extent which only a part of those present in Norwegian oil are. Nearly all of the acids in sample E are unsaturated, as shown by the figures obtained from the unsaturated fatty acids themselves separated from the oil. These figures were on three different occasions 173.3, 170.5, 165.6. From the sample E Mr. Branch isolated fatty acid containing eighteen carbon atoms, with double bonds, thus resembling an acid separated from the liver by Hartley containing twenty carbon atoms and four double bonds.

Speaking of this acid, Leathes says: "There are, indeed, clearly yet things to be learnt with regard to the fatty acids of the liver. But a beginning has been made and indications exist as to the nature of the biological significance of some of the facts that have been already determined. To assign its place in the metabolism of the fats to this remarkable acid, however, is a task that we are not as yet equipped to undertake. It may be a product of that synthetic activity of which there is some evidence in the liver, but it is impossible to say.

It is thus seen that cod-liver oil is quite a different type of fat to any which occurs normally in food. It contains little, if any, phosphatide, is not a mixture (when prepared in an atmosphere of CO_2) of saturated and unsaturated fats, but is composed almost entirely of unsaturated fats.

The degree of unsaturation is far above anything occurring in the food fats, and is comparable only to the fats which the liver prepares for the immediate needs of the body. In the oil itself they occur as glycerides, and as such are fairly stable bodies, but the acids when liberated are highly reactive, as we have shown by the ease with which they undergo polymerization. It is thus clearly seen that cod-liver oil, when taken into the intestinal tract and split up in the process of digestion, liberates very highly reactive bodies, and, further, bodies which are easily assimilated. It is not difficult to understand that such bodies may have the marked effect on metabolism shown by us.

We have confirmed the observation that there is a relation between the taste of the oil and the number of hydroxyl groups it contains. Sample E contained only traces of hydroxyl, and had far less taste than any of the other oils examined.

There is a marked difference between the oil prepared under conditions which do not allow of its oxidation and those not so prepared. In the former the amount of unsaturated acid is much greater, there is less free fatty acid, and less impurities. It has also less taste.

Conclusions.

1. Clinical evidence shows that cod-liver oil has beneficial effects in phthisis.
2. It has marked action on fat absorption and nitrogen retention.
3. It is possible that the fatty acids of the oil, when they reach the tissues, have direct influence on the tubercle bacillus.
4. Of the oils on the market, the forms which are prepared under conditions which prevent oxidation have the greater amount of unsaturated fatty acid compounds to which the therapeutic effects can be attributed.
5. The oils prepared under these conditions have the least taste.

REFERENCES.

[1] The Influence of the Unsaturated Fatty Acids in Tuberculosis, BRITISH MEDICAL JOURNAL, vol. ii, 1909, p. 1120. [2] On the Nature of the So-called Fat of the Tissues and Organs, *Bio-Chemical Journal*, vol. iv, p. 455. [3] Lipoid Material in Diabetic Blood, *Bio-Chemical Journal*, vol. ii, 1907, p. 20.

DISCUSSION.

Dr. GORDON SHARP (Leeds) said Dr. Williams's paper was most timely, for it placed cod-liver oil on a sound scientific basis. Despite the vigorous fresh-air campaign now in progress, it was impossible to dispense with the use of cod-liver oil. This needed to be emphasized, for there was a disposition among many physicians to rely on fresh air and to leave off this old-fashioned but useful remedy. The speaker could confirm Dr. Williams's statement that patients always benefited under cod-liver oil. This took place even when the patients lived in their former unfavourable conditions. The oil appeared to stimulate metabolic processes quite apart from its pure fat value.

Professor J. HILL ABRAM (Liverpool) referred to the general belief in the value of cod-liver oil, even when not prepared in an atmosphere of carbon dioxide. He suggested a series of experiments, using tissue media as a basis for the comparison of the action of unsaturated and saturated acids in their effect upon the growth of the tubercle bacillus.

Dr. WILLIAMS shortly replied.

THE ACTION OF SYMPHYTUM OFFICINALE.

By CHARLES J. MACALISTER, M.D., F.R.C.P.,

Honorary Physician to the Royal Southern Hospital and to the Royal Liverpool Country Hospital for Children, etc.

SINCE the publication of my paper, entitled "A New Cell Proliferant," in which was related the discovery that the ancient fame of the *Symphytum officinale*, or common comfrey, as a vulnerary depends upon the richness of its rhizome in allantoin, a number of observations have been made with this substance which in a measure help to explain the nature of its action.

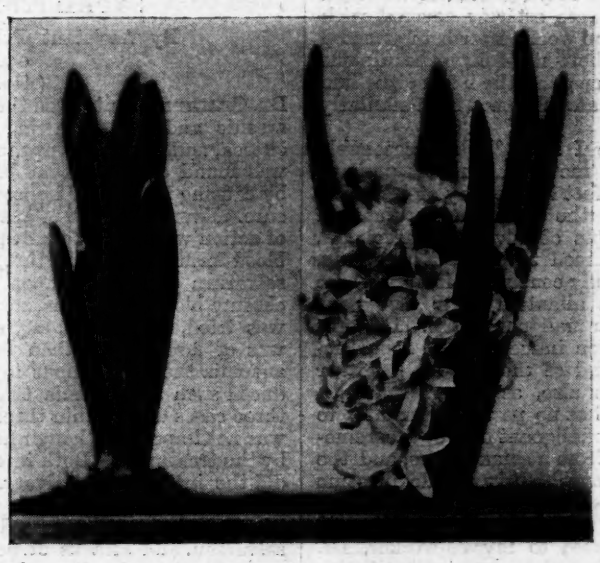
Its utility in the treatment of ulcerative conditions and of burns has been confirmed by the testimony of numerous observers, who in the descriptions of their cases have frequently referred to the marked way in which it has cleaned up the bases of the ulcers even when they have been septic and sloughy. This cleaning-up process is, of course, essential if healing is to occur, because healthy cell proliferation will not take place over septic surfaces, and is conducting an investigation concerning the action of allantoin the first thing to determine was whether it was primarily destructive to micro-organisms or promoted this effect by rendering the cells immune to them and their surroundings unsuited for their growth and development.

Some simple experiments quickly demonstrated that, far from being a poisonous antiseptic, it possesses no toxic qualities, and rather favours than inhibits the growth of organisms. In August, 1911, during a cruise to the Shetlands in Professor Herdman's yacht, *The Runa*, I was able to make a number of observations concerning the effects which followed the placing of samples of the plankton gatherings (which were constantly available during tow-netting operations) in sea-water containing varying amounts of allantoin. The object of this was to discover whether the allantoin was destructive to these small and simple forms of animal and vegetable life which

are found in the plankton. Owing to the active way in which they move about, the most easily observed creatures were the copepoda, but there were many other forms which could easily be watched with the naked eye. An excess of allantoin was placed in a quantity of sea-water and thoroughly shaken up, so as to make as saturated a solution as possible, and of this quantities were added to newly drawn sea-water in proportions varying from 1 in 8, 1 in 7, and so forth up to saturated strength, controls of pure sea-water being employed in every series of experiments. The net result of a considerable number of observations was that the animalculae observed lived for nearly as long a time (about two days) in the saturated and other solutions as they did in the controls, and from this it appeared that allantoin was not appreciably inimical to the forms of life which were experimented upon.

On my return to Liverpool Dr. Alfred Adams, working in the Bio-chemical Laboratories of the University, kindly made some experiments with various culture media, containing 0.2 per cent. of allantoin, and on November 1st, 1911, he reported that the *Bacillus coli*, staphylococci, streptococci, and tubercle bacilli were not retarded in their growth and multiplication by it.

Allantoin, then, is not an antiseptic in the usual acceptation of the term, and its action in this respect must depend upon some influence brought to bear upon the cells, whereby their resistance and stability and immunity are established and their proliferation promoted. As stated in the paper referred to, allantoin is found in parts which are related to active growth and development both in animals and plants. For instance, it is plentiful in the fetal allantois, in the rhizomes of plants at certain seasons of the year, and in the young buds and germinating parts of those which are growing and developing; in the comfrey rhizome it is abundant in the spring and autumn, the seasons during which it should be always gathered for therapeutic purposes, whereas in samples taken during June of this year, when the plant was well grown and in flower, the rhizomes contained a hardly appreciable quantity of it, but it was present in the leaves and young shoots. This important fact may be regarded as evidence that the plant withdraws allantoin from its storehouse in the rhizome, and utilizes it for purposes of cell proliferation.



The presence of allantoin in the storehouses and growing parts of plants, as well as its relation to the fetus in the allantois and in milk—the food of the rapidly growing infant—added to the clinical evidence that it promotes healing in ulcerative conditions, naturally led to the suggestion that experiments made upon plants might afford some information, confirmatory or otherwise, of its proliferative properties or functions. By my request Mr. Coppin planted a large number of hyacinth bulbs (the growth of which had been started in the dark) in solutions of allantoin varying from 0.1 to 0.5 per cent., and it was found that the growth of the roots was inhibited in a ratio proportionate to the amount of allantoin in the solution—that is, the stronger the solution the less was the amount of growth in the root.*

Notwithstanding this diminished root growth, which was confirmed by microscopic examination, the flower stems of the plants grew in a stunted way, but none had blossomed at the end of twelve weeks, and it therefore appeared that allantoin had a detrimental effect upon vegetable cell growth when added to the water in which the bulb was planted. This was disappointing, but an interesting development now ensued. It seemed evident that in plants the allantoin or other proliferative agent is stored for use in the rhizomes, buds, barks, and germinal parts, and it is probably elaborated there as the result of some metabolic process. There is no allantoin normally in water or in the earth in which bulbs are grown, but it or some kindred substance is formed and stored in the bulb, or, in the case of other plants, in the rhizomes or roots, to be drawn upon as required for the advance of the processes of growth and development. This being the case, the suggestion naturally arose that one might try the effect of injecting a solution of allantoin into the bulbs, and a number of experiments were conducted on these lines, the first of which was made in a more or less casual way as follows:

A child in the Royal Southern Hospital had two earth-grown hyacinths in a pot. They had grown quite unequally, one having developed well with the commencement of a flower, the other being feeble, short, and showing no signs of blossom.

About 15 minims of allantoin solution (0.4 per cent.) was injected on several occasions into the bulb of the latter, with the result that rapid growth ensued and it overgrew its more vigorous neighbour.

This experiment excited the interest of the Sister of the ward (Miss Archer) and of the Matron (Miss Jolly), and they gave similar injections into the bulbs of a considerable number of plants in about equal stages of growth. Controls were used, some being injected with quantities of water equal in amount to the quantities of allantoin solution given, others being grown without any injections, these experiments being conducted in earth-grown bulbs. In addition, some hyacinth bulbs which had just started growth, the shoots being about ½ in. high, were planted in water, and some of them were injected every third or fourth day with allantoin solution, some with water only, the remainder being controls, and the same results were obtained in every case. The allantoin evidently acted as a cell proliferant, forcing the growth of the shoots, and especially of the flowers (for it was noticeable that the plant often blossomed with comparatively little leaf growth), commonly before their untreated neighbours had reached anything like the stage of flower production. The water-injected plants grew better than the uninjected ones, but nothing like so rapidly as those to which allantoin had been added.

These experiments were repeated many times with tulips, lilies of the valley, and other plants, care being taken that they were planted at the same time, in the same tubful of earth, and under the same conditions of temperature and surroundings.

I show some pictures illustrating the way in which injected plants flourished as compared with the controls. [One is reproduced on this page.]

Professor William Carter experimented with shallots and obtained similar results, only he watered earth-grown plants with allantoin solution against controls where water only was used. It must be remembered that in this case the water enters into the bulb from above, soaking in between the fleshy tissue layers as well as being absorbed by the roots. In the water-grown plants which were injected, it was noted that with the increased growth of the shoots there was sometimes a diminished growth of the roots, which were considerably longer and stronger in the controls.

This led to the impression that perhaps the roots were not so necessary to the plant if its water as well as the proliferant content were kept high, and in order to test this point a number of hyacinth bulbs were selected which had shoots ½ in. high but no visible roots. Some of these I injected with allantoin solution, some with water, others being uninjected, and they were simply placed upon a plate without earth or water. At the end of seven or eight days the little shoots of the treated plants were opening out into leaves. The water-injected bulbs also

* Mr. Coppin, M.Sc., is publishing a paper describing his experiments fully in the coming number of the *Bio-Chemical Journal*. The type of experiment was suggested by a paper by Professor Benjamin Moore and Herbert Roaf and Robert E. Knowles, *Bio-Chemical Journal*, vol. iii. Nos. 6, 7, and 8.

lived, but did not grow so well, whereas the shoots of the controls browned at the tips and shrivelled up. Not a sign of roots made their appearance in any of these plants.

From these experiments one gathers the impression that allantoin is a substance which is capable of being utilized by vegetable cells in connexion with their proliferative processes, just as there seems to be proof that it has proliferative properties in connexion with certain animal cells, if one may judge by the way in which it promotes healing in the chronic and acute ulcerative conditions which have been recorded or communicated, and in both cases the reproduced cells are normal, and resemble those from which they took their origin.

The next point to determine was whether allantoin had any influence upon malignant cell growth. A good many cases have been recorded where cancerous or sarcomatous growths have benefited by treatment with comfrey. There is a very striking one mentioned by Professor William Thomson in a paper entitled "Some Surprises and Mistakes," published in the BRITISH MEDICAL JOURNAL of 1896, in which a man, who had a large recurrence after removal of the jaw for sarcoma, treated it himself with comfrey poultices, and it atrophied and disappeared. The author says, "I know nothing of the effects of comfrey root, but I do not believe that it could remove a sarcomatous tumour." The very first case in which I employed allantoin was a large rodent ulcer, which became entirely covered with epithelium in the course of some weeks after being treated with it. In this latter case there must have been some special circumstances which enabled the new epithelium to grow in, because other rodents have not been similarly benefited by it.

Concerning the former case I can offer no reasonable explanation, but they both led me to think that perhaps allantoin might have some effect in the way of lessening the malignant tendencies of the cells. Accordingly I proceeded to use it in a couple of advanced sarcomata, with the result that they seemed to grow, if anything, rather faster than before, and my conclusion is, from these and other observations, that allantoin is a dangerous substance to give or to apply in malignant cases. It seems to be a general proliferant which influences the reproduction of normal animal cells and of those of malignant or reproductive types, and it is also, as I have shown, a vegetable cell proliferant. It has no power apparently to produce a somatic cell from a malignant one, or a carcinomatous or sarcomatous cell from a somatic one, and the inference is that it is an agent which has to do in some general way with the activities of nucleic acid.

In this connexion a study of the structure of nucleic acid is important, and I am indebted to my co-worker, Dr. Titherley, for facts concerning the chemistry of this substance and the possible relationship of allantoin to it. Nucleic acid ($C_{43}H_{67}O_{30}N_{15}P_4$) consists of four meta-phosphoric molecules, having linked on to them one molecule of a pentose (discovered by Emil Fischer to be ribose, a substance difficult to obtain, and therefore difficult to experiment with), one molecule each of the purine bases, adenine and guanine, and one each of the pyrimidine derivatives, thymine and cytosin. It is to these two latter (purine and pyrimidine) groups that allantoin is related. Both of them yield uric acid in the course of their chemical metabolism, and uric acid in turn when oxydized yields allantoin, so that allantoin may be said to have certain relationships to nucleic acid, and I expect that bio-chemically it will prove to be a very important relationship, for reasons already referred to. It seems reasonable to suppose that cellular stability and resistance may depend upon the efficient building up of nucleic acid. There is no use in giving or applying nucleic acid (or nucleo-protein) itself in cases where it is desired to build up that substance in the cells; they cannot make use of the fully formed material, but require to assimilate its constituents to build it for themselves, and if cellular stability and resistance and immunity depend upon its proper construction and functions, we must try to supply the bricks for building the edifice.

During the past six or seven months I have been experimenting with meta-phosphoric acid alone and in combination (mixture) with allantoin, more especially in tuberculous sinuses and skin infections, in order to discover whether cell resistance and stability could be strengthened and healing promoted. It has been administered internally and given hypodermically into the tuberculous tissues, syringed into sinuses and into abscess cavities; and there is no doubt that it has done much good, the tissues have softened down and healing has been effected in long-standing cases, and it seems to me to be likely that a principle of this kind may lead to a change in our conception of the methods of treating tuberculous and perhaps some other infective conditions. There is great scope for work in the investigation of the therapeutic effects of these nucleic acid derivatives and constituents, some of which we have now under investigation, and I hope on a future occasion to be able to add something further to our knowledge concerning them. There is just one word to add with reference to the therapeutics of allantoin. I have already referred to its presence in milk, the food of the infant and rapidly growing child. Experiments conducted on the lines of marking the increase of weight, if any, of children suffering from wasting and rickets on a known milk mixture, and then noting it after the addition of allantoin to the milk, indicate that it promotes their growth and nutrition.

THE COMPARATIVE VALUE OF ARSENIOUS ACID AND SALVARSAN IN NON-SYPHILITIC DISEASES.

By Assistant Professor CHARTERIS,
Glasgow.

(*Abstract.*)

DR. CHARTERIS contrasted the therapeutic value of inorganic arsenic and salvarsan in Hodgkin's disease, leukaemia, chorea, and pernicious anaemia. In all these conditions it was found clinically that inorganic arsenic was of value by causing temporary improvement though rarely complete cure. Even in chorea relapses were common. The method of action was quite unknown. There was an effect on the blood-forming tissue in the marrow. On the theory that salvarsan might act upon the unknown cause of these obscure blood conditions, intravenous injection of salvarsan was tried, but without any results which indicated that it was of more value than inorganic acid. In pernicious anaemia the intravenous injection of salvarsan had produced such an anaemia that the patients in two out of three cases died within three weeks, and the third patient was not improved, though he apparently was not damaged by the treatment. Both in chorea and leukaemia not only did the injection of salvarsan not cure the disease, but it even did not prevent the appearance of complications. Thus, in chorea an endocarditis ran a normal course after salvarsan, and attacks of perisplenitis still occurred in a case of leukaemia after salvarsan treatment. It was found that in rabbits salvarsan injection produced a mild stimulatory effect upon the bone marrow analogous to that induced by inorganic acid.

COMBINED SECTIONS OF STATE MEDICINE AND BACTERIOLOGY.

Dr. A. K. CHALMERS, President of the Section of State Medicine, in the Chair.

DISCUSSION ON THE VARIETIES AND SIGNIFICANCE OF B. COLI IN WATER SUPPLIES.

OPENING PAPERS.

I.—A. C. HOUSTON, M.B., D.Sc.Edin.,
Director of Water Examination, Metropolitan Water Board.

Introduction.

IN illustration of the potentialities of the *Bacillus coli* test, two series of experiments were carried out, and the results are shown in Tables I and II.

TABLE I.—*Delicacy of the B. Coli Test: One Part of Crude Sewage was Added to 100 Million Parts of Sterile Water.*

By using 10,000 c.cm. of the infected water for cultural purposes the following results were obtained:

Experiment 1.—*B. coli* (lactose +, indol +) isolated without any difficulty.

Experiment 2.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 3.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 4.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 5.—Here the result was a failure, the reason being that the sewage used in this experiment happened to be very weak bacteriologically, containing only 1,000 *B. coli* per c.cm.
Experiment 6.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 7.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 8.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 9.—*B. coli* (lactose +, indol +) isolated without any difficulty.
Experiment 10.—*B. coli* (lactose +, indol +) isolated without any difficulty.

The *B. coli* test is thus so delicate that one part of a normal sewage, or its bacteriological equivalent, can be detected in 100 million parts of sterile water.

Table I indicates the remarkable delicacy of the test. It will be noted that in nine out of ten experiments typical *B. coli* (lactose +, indol +)* were isolated from a known mixture containing 1 part of sewage in 100 million parts of water. The mixture was made by adding what represented 0.0001 c.cm. of sewage to 10,000 c.cm. of sterile tap water, contained in a large sterile bottle, thereafter adding a concentrated suitable sterile nutrient medium and incubating the mixture, and subsequently making subcultures in the ordinary way. With many sewages, even 0.00001 c.cm. would have sufficed (1 in 1,000 million) to yield a positive result, but the particular sewage used in these experiments is known to contain on certain occasions relatively few *B. coli*.

TABLE II.—*Absence of Typical B. Coli (Lactose +, Indol +) from 10,000 c.cm. of a Deep Well Water.*

Experiment 1.—10,000 c.cm. of the well water were poured into a large sterile bottle, a suitable enrichment of sterile bile-salt sugar medium was added, the bottle and its contents were placed in a warm water bath at 37° C. for several hours and then finally transferred to the incubator (at 37° C.). After three days' incubation subcultures were made. No typical *B. coli* (lactose +, indol +) or *coli*-like microbes of any kind were isolated.
Experiments 2 and 3.—Same procedure as in Experiment 1, with similar negative results.
Experiment 4.—Here microbes were isolated which yielded traces of gas in lactose cultures, but no indol in peptone-water cultures. No typical *B. coli* (lactose +, indol +) were found.
Experiment 5.—Same as Experiment 4.
Experiments 6 to 10.—Results quite negative, as in Experiments 1 to 3.

Table II illustrates a related picture. Here it will be observed that when dealing with a deep well water known to be very pure, in ten instances the attempt to recover typical *B. coli* failed, even when as much as 10,000 c.cm. were submitted to cultural tests. What do these results signify? They signify that the well water in question could not have been contaminated with sewage or its bacteriological equivalent even in the small proportion of 1 part of the former in 100 million parts of the latter. It is, of course, impracticable in general cases to use as much as 10,000 c.cm. for cultural purposes, but 100 c.cm. can always be employed, and so it may be said that in practice the test can readily detect 1 part of sewage or its bacteriological equivalent in 1 million parts of water. With a strong sewage the test may even detect 1 part in 10 million parts.

Requisites for Bacterial Tests of Pollution of Water.

Three main requisites for reliable tests of contamination are:

(1) To begin with the microbe indicator must be superabundant in excremental matters. (2) It must be normally absent or present only in comparatively small numbers in water free from undesirable pollution. (3) It should be a decadent microbe when divorced from the animal body.

1. As regards the first requisite, it is probably unnecessary here to embark on a description of the work I have done in estimating the numerical proportion of *B. coli* in excremental matter. Those familiar with my writings on this subject will accept the statement as being probably an underestimate that there are 100,000 *B. coli* per c.cm. of crude sewage and 1,000,000 *B. coli* per gram of human faeces.

2. With respect to the second desideratum, it is superfluous to go into the matter fully, but a few representative examples may be given:

Deep Well Water.—Taking 1,331 samples of Kent deep well waters, 94 per cent. showed no typical *B. coli* in 100 c.cm.; similarly with 458 samples of Lee Valley deep well waters 91.4 per cent. contained no typical *B. coli* in 100 c.cm.

Deep Sea Water.—34 samples collected off the West Coast of Scotland contained no typical *B. coli* in 100 c.cm.; 8 samples collected outside the Firth of Forth contained no typical *B. coli* in 100 c.cm.; 8 samples collected off the Norfolk coast contained no typical *B. coli* in 100 c.cm. Six of the Norfolk sea-water samples were also tested in the very large amounts of 1,000 c.c.m. cultures, and the results were likewise negative.

Deep Sea Shellfish.—A hundred and twenty cultures, amounting in the aggregate to 382 c.cm., were made from sixty-six deep sea oysters collected off the Norfolk coast. With a single exception none of the cultures yielded any typical *B. coli*. The microbe associated with the apparently positive result was not altogether typical when further tested in neutral red broth and litmus milk cultures.

I may also refer to the numerous experiments on sea-fowl and sea-fish described in my report to the Local Government Board, 1903-4. In these investigations I found that whereas the intestines of the ordinary gull harboured *B. coli* in enormous numbers, the guillemot, which is a clean feeder, did not apparently contain this microbe. Further, the intestines of crustaceans and fish—for example, shrimps, whelks, crabs, gurnets, cat-fish, whitings, soles, plaice, dabs, brill, skate, and dog-fish—dredged from the sea in localities remote from sewage pollution usually contained no typical *B. coli*. The foregoing are a few instances of the absence, practically speaking, of *B. coli* from unpolluted substances. It may, however, be at once acknowledged that between the two extremes of (1) and (2) lie many cases of materials which admittedly do contain *B. coli*, but which, nevertheless, appear to be perfectly harmless if ingested.

3. As regards the third desideratum, it is possible that under special circumstances, the *B. coli* may normally retain its vitality for considerable periods. This fact, however, does not invalidate the rule that *B. coli* outside the animal body is usually known to be a microbe with decadent attributes—for example, at 20° C. *Bacillus coli* dies rapidly (three to nine days) both in sea-water and tap-water.* The experiments, however, were carried out with cultivated organisms in 1903 and before I had adequately appreciated the fact that low temperatures have a marked effect in prolonging the vitality of *B. coli*. As regards uncultivated *B. coli*, a very large number of experiments at a later date with Thames and Lee raw river water (10 c.cm. amounts) showed that on the average *B. coli* disappeared within eleven days at laboratory temperatures.

Turning next to open-air conditions, in 1903 I had occasion to examine the Lee stored water feeding the sand filters at Lee Bridge, and was struck by the comparative absence of *B. coli* notwithstanding the fact that in the Lee river antecedent to storage this microbe is present in considerable abundance. At a later date, the matter has again been studied in great detail, but time only permits of my giving the following few particulars.

TABLE III.

Presence of Typical *B. coli* (Lactose +, Indol +) in :—	Raw Thames.	Raw Thames Water after Storage in the Lambeth, Chelsea, and Staines Reservoirs.		
		Lambeth, nominally 14 Days' Storage.	Chelsea, nominally 15 Days' Storage.	Staines, nominally 95 Days' Storage.
	Per Cent.	Per Cent.	Per Cent.	Per Cent.
100 c.cm. and less ...	99.9	83.7	57.2	66.2
10 c.cm. and less ...	97.7	51.2	32.5	32.5
1 c.cm. and less ...	83.1	23.7	13.4	9.6
0.1 c.cm. and less ...	48.3	7.5	3.3	None
0.01 c.cm. and less ...	10.1	None	1.1	None

* To save repetition it may be here stated that throughout the paper typical *B. coli* signifies *B. coli* yielding gas in lactose cultures and indol in peptone-water cultures.

* See page 102, Fourth Report of the Royal Commission on Sewage Disposal, vol. iii.

The table shows very clearly the striking reduction in the number of *B. coli* when raw river water is stored in large storage reservoirs.

Enough, perhaps, has now been said under (1), (2), and (3) to establish their general accuracy, and it is convenient at this stage to consider the significance of the *B. coli* test in its negative and positive aspects.

Negative Aspects of the B. Coli Test.

In its purely negative aspect the value of the *B. coli* test is almost outside the pale of criticism. Reference may again be made to Table II, which indicates that the well water in question, at the time when the samples were collected, contained no typical *B. coli* in 10,000 c.cm. of water. In other words, it contained either none at all or less than 1 part of sewage or its bacteriological equivalent in 100 million parts of water. Now, when a water contains no typical *B. coli* in 100 c.cm., it may be said to be 10 million times less impure than sewage* or its bacteriological equivalent. Of course the *B. coli* test in its negative aspects is only of significance inferentially—that is, one cannot prophesy from negative results at any one time the absence necessarily of positive results in the future. Further, improperly protected water may be contaminated with the urine of a typhoid carrier containing no *B. coli* but myriads of typhoid bacilli.

Positive Aspects of the B. Coli Test.

Here obviously we are treading on debatable ground, and it is not always easy to avoid, on the one hand, condemning waters which are really safe or passably safe, and, on the other hand, passing waters which are actually or potentially unsafe for domestic use. Not being a believer in blindfold bacteriology, I consider that *B. coli* results should always be interpreted in relation to the topographical conditions. It is just as foolish for the topographist to ignore the findings of the bacteriologist as it is for the bacteriologist to pay no attention to local conditions.

Suppose, for the sake of argument, we condemn a water containing typical *B. coli* in 1 c.cm.,† this means that we are objecting to its quality because it contains $\frac{1}{10,000}$ part of sewage or its bacteriological equivalent.‡

This is a lenient standard. I think, indeed, there cannot be very many waters yielding *B. coli* in 1 c.cm. in a majority of representative samples which cannot be shown to be undesirably polluted. On the other hand, I think it must be admitted that there are some waters which may just fail to yield positive results with 1 c.cm. amounts, and yet which may not be altogether free from objection.

Suppose the above standard be made ten times more severe, and we object to a water containing typical *B. coli* in 10 c.cm. of water. It is obvious that there is more risk of condemning supplies which are reasonably safe, and less risk of passing others which are not above suspicion than is the case with a 1 c.cm. standard.

The standard may be made still more severe by condemning waters yielding positive results with 100 c.cm. amounts, but this, of course, involves the danger of passing an unfavourable opinion on not a few supplies, which, as judged by other tests and standards and by practical experience, are even free from suspicion. With this 100 c.cm. standard the risk of passing supplies which ought to be condemned is reduced to a minimum. The 100 c.cm. standard is the one which I adopted a good many years ago as a counsel of perfection, but I am far from saying indiscriminately that all waters which infringe the standard are of necessity unsuitable for domestic use. For example, surface waters from regions remote from the possibilities of sewage pollution, and from land which is not under cultivation, may yield *B. coli* in 100 c.cm. or 10, or possibly even 1 c.cm., and yet experience has shown that these same waters may be perfectly safe for drinking purposes. On the other hand, when dealing with well waters so situated as not to be wholly cut off from all sources of undesirable pollution, or with surface waters, which, by reason of their suspicious origin, have to be subjected to some form of purification treatment, no effort should be spared to bring such supplies into the position indicated as safe by the proven absence of *B. coli* from as much as 100 c.cm. of the water under investigation. That the standard is not at all unattainable is shown by the present position of the metropolitan water supply, which in 1911 yielded from somewhat unpromising antecedents the following results:

All London Waters, 1911.

Number of samples examined, 7,091.
Percentage number of samples containing no typical *B. coli* (lactose +, indol +), 77.6 per cent.

In 1907, in conjunction with Dr. M. H. Gordon, I examined 215 tap-water samples from 26 large towns; 57.2 per cent. of the samples contained no flaginac *B. coli* in 100 c.cm. of water and seven towns yielded no flaginac *B. coli* in 100 c.cm. out of 53 samples examined.

I think bacteriologists would always be justified in urging some objection on purely bacteriological grounds to all waters which do not yield, in a majority of representative samples, negative results as regards the typical *B. coli* when 100 c.cm. of water are submitted to cultural tests, but not necessarily in practice in condemning such waters without taking into account all the circumstances pertaining to the particular case under consideration. There is no inconsistency or weakness involved in admitting that the *B. coli* test, like all tests, has its limitations. On the other hand, the topographist who affects to ignore the findings of the bacteriologist is as open to censure as the clinician who, despite the findings of a clinical research laboratory or the revelations of the *post-mortem* room, refuses to enlist in his services the microscope or the test tube as most useful workaday adjuncts in his practical life.

The B. Coli in Relation to Purification.

The *B. coli* test, as an indicator of the efficiency of purification processes, stands without a peer, whether in judging the efficiency of a filtration process, or the adequacy of storage of impure water, or, before all, in determining whether a sterilization process is really effective. In the case of the metropolitan water supply over three-quarters of the samples of raw river water (Thames and Lee) contain typical *B. coli* in 1 c.cm. (or less), whereas over three-quarters of the samples from the same source, but after storage and filtration, do not contain this microbe, even in 100 c.cm. Whatever may be the demerits of London's sources of supply, the obvious and fair bacteriological inference is that the purification processes decrease the risks a thousandfold.

The B. Coli Test in Relation to other Tests.

In human faeces the results as judged by (*a*) the total number of bacteria (gelatine), (*b*) blood-heat bacteria (agar), (*c*) excremental bacteria (rebipelagar), and (*d*) typical *B. coli* (lactose +, indol +) are almost identical for obvious reasons. In sewage and sewage effluents, (*a*), (*b*), (*c*), and (*d*) diverge, the numbers being in the order named. As we pass from sewage to waters of increasing degrees of purity, we find usually the amount of divergence increasing—that is, relative to each other the harmless bacteria become more prominent than those associated more or less closely with undesirable pollution. These comparisons are of interest, but of far greater importance is the relative merits of the different bacteriological tests. Beyond all question, and without disparaging any other supplemental or corroborative test, the *B. coli* test, assuming that the test is properly applied and the results interpreted with discretion, is the "sheet anchor" of practical water bacteriologists.

Objections urged against its Value.

Most if not all the real limitations of the *B. coli* test were admitted, and, in fact, originally brought forward by the very men whose practical experience still leads them to persevere with the test. Perhaps the two most common objections are that *B. coli* is abundant everywhere, and that it multiplies in water. Neither statement is true in my experience. Again, it is said that as *B. coli* is non-pathogenic the test has no value. But a water is not condemned because it contains *B. coli* as *B. coli*, but because the presence of *B. coli* indicates the possible

* On the basis of 100,000 *B. coli* per c.cm. of sewage.
† That is, in a majority of samples, taken to be fairly representative of the average conditions of the supply in question. This note applies to the immediately succeeding paragraphs.
‡ On the basis of 100,000 typical *B. coli* per c.cm. of sewage.

presence of accompanying microbes of truly pathogenic sort. Further, it is correctly objected that the *B. coli* of man and that of the lower animals are practically indistinguishable. Undoubtedly the excreta of the lower animals are much less objectionable than those of man; but some of the diseases of animals are communicable to the human, and the ingestion of any kind of filth is surely to be avoided if reasonably practicable. Even in cases of gross indisputable sewage pollution the *B. coli* results have, on the above-stated grounds, been sometimes discredited, as if, forsooth! the lower animals had acquired the monopoly of our waterclosets to the exclusion of human beings. Further, it is argued that until bacteriologists are agreed on the definition of *B. coli* the test is of small value. But bacteriologists disagree only as regards the interpretation to be put upon the refinements of the test; and surely, as such disagreement only stimulates toward further research, it is to be hoped that it will continue.

Some bacteriologists think that they can get nearer the actual truth by a rigid classification, and others by a more elastic interpretation of their results, but it becomes at once apparent how relatively unimportant these differences really are if we look at the matter broadly and quantitatively. Practically every one is agreed that a microbe of "lactose +, indol +" attributes is probably of excremental origin, and the distribution of such microbes in Nature has already been dealt with. It has been shown that it may be absent from as much as 10,000 c.cm. of a very pure water and present in $\frac{1}{10,000}$ c.cm. of sewage.

Further proof has been afforded that if 10,000 c.cm. of sterile water be inoculated with $\frac{1}{10,000}$ c.cm. of sewage, "lactose +, indol +" microbes can be recovered without any difficulty. In other words, the test can readily detect 1 part of sewage in 100 million parts of water, or even 1 part in 1,000 million parts in the case of strong sewage.

Classification of Results.

I think a majority of bacteriologists have now adopted some such classification as was suggested by me in 1902.*

First Class.—No "lactose +, indol +" microbes even in 100 c.cm. of water; 10,000,000 times less impure than sewage or its bacteriological equivalent.

Second Class.†—No "lactose +, indol +" microbes in 10 c.cm. of water; 1,000,000 times purer than sewage or its bacteriological equivalent.

Third Class.—No "lactose +, indol +" microbes in 1 c.cm., but present in 10 c.cm. of water; 100,000 times purer than sewage or its bacteriological equivalent.

Fourth and succeeding classes on the same decimal basis.

In addition, each bacteriologist very properly likes to fortify his judgement by the application of additional tests, interpreted in whatever manner his experience leads him to think best. My point is that the quantitative estimation of the number of "lactose +, indol +" microbes is common ground for practically all bacteriologists, and the classified results based on this standard of measurement are found to be of the utmost value in connexion with water analysis.

Varieties of Typical B. Coli (Lactose +, Indol +).

It is quite easy by employing only six additional tests (adonite, cane sugar, dulcite, glycerine, raffinose, salicin) further to subdivide the "lactose +, indol +" *B. coli* of sewage into at least nineteen apparent varieties.

It will be noted that out of 100 specimens of typical *B. coli* (lactose +, indol +) isolated from sewage, there occurred 19 apparent varieties, though the proportion of some of these to the whole was very small (Table IV).

It may be convenient at this stage to refer to Tables V, VI, and VII, illustrating apparent varieties of lactose +, indol + *B. coli* isolated from water, sewage, sewage effluents and faeces. I am not prepared, however, to attach to these varieties any definite place in a scale representing degrees of undesirable pollution.

TABLE IV.—*100 Specimens of Typical B. Coli (Lactose +, Indol +) Isolated from Sewage.*

+ = Gas.

Group.	Adonite.	Cane Sugar.	Dulcite.	Glycerine.	Raffinose.	Salicin.	Per Cent.
1	+	+		+	+	+	4
2	+			+		+	10
3	+						1
4	+	+	+	+	+	+	2
5	+					+	1
6	+			+			1
7		+	+	+	+	+	22
8		+	+	+	+		13
9		+		+	+		8
10		+	+	+			1
11		+		+	+	+	2
12			+	+		+	13
13			+	+			9
14			+	+	+	+	2
15			+	+	+		2
16				+			4
17				+		+	3
18				+	+		1
19		+			+		1

		Per Cent.
Adonite Group ...	1, 2, 3, 4, 5, 6...	19
Cane Sugar Group	1, 4, 7, 8, 9, 10, 11, 19	53
Dulcite Group ...	4, 7, 8, 10, 12, 13, 14, 15	64
Glycerine Group ...	1, 2, 4, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18...	97
Raffinose Group ...	1, 4, 7, 8, 9, 11, 14, 15, 18, 19	57
Salicin Group ...	1, 2, 4, 5, 7, 11, 12, 14, 17	59

No definite liquefaction of gelatine, 30 days at 20° C.

METHODS.

Primary Medium.—In the case of all but very impure waters it is necessary to use enrichment methods and liquid media for primary test purposes. Though as a result of comparative tests I obtained the best actual results with beef broth, next with McConkey's glucose bile salt medium, and then with his lactose bile salt medium, yet the use of bile salt media saves so much time and trouble that, generally speaking, they are entitled to preference. I am a little doubtful about the choice of a fermentative substance. A positive result with lactose is a much better presumptive sign of contamination than a positive result with glucose. On the other hand, both are indicators of potential danger, but a negative result with a lactose medium may lull suspicion, which would be aroused by the use of a glucose medium.

Bacteriologists are too frequently called upon, or expected, to form a judgement from the examination of a single sample from an unknown source of water supply. Suppose a negative result with 100 c.cm. and lesser amounts is obtained when using a lactose medium. On the basis of this result alone, there is no option to pronounce other than a favourable opinion. Had glucose been utilized, a positive presumptive result might have been obtained, although possibly no typical *B. coli* (or only non-typical *B. coli*) resulted from the subcultures, in which case a guarded statement would then be made, properly leading to the collection of further samples, which could be shown to contain possibly completely typical *B. coli*.

In the great majority of waters, although there is no evidence that a change in the hygienic quality of the water has actually occurred, yet striking variations occur from day to day, when dealing with the same volume of water, in the matter of isolation of glucose and lactose fermenting microbes. In the case of the metropolitan water supply, we are dealing with Thames, Lee, and New River filtered water and a considerable number of well waters. In the case of only one well has a positive result as regards fermentation not been followed, sooner or later, by a positive

* Report of the Medical Officer, Local Government Board, 1902-3, Opp. B. No. 5, p. 559. Also Fourth Report of the Royal Commission on Sewage Disposal, vol. iii, p. 107.

† As suggested in my report to the Local Government Board, each of these classes may be subdivided into ten subclasses. For example: +100-90, +90-80, +80-70, +70-60, +60-50, +50-40, +40-30, +30-20 +20-10.

result in respect also to lactose, and it is probably only a question of time when the bacteriological record of this well will follow that of the other wells and river-derived supplies. In my experience, the lactose primary media would seem to have the greatest usefulness in summer and hot and sunny weather, whereas in winter and cold weather the glucose primary media yield the largest number of typical *B. coli*.

I have found it a useful plan to work with a lactose primary medium, incubating the cultures for about twenty to twenty-four hours at 40° to 42° C., and then examining them for gas production. If there be any gas production, the cultures are put back in the same incubator for another day. If, on the other hand, the results are negative at twenty to twenty-four hours, a few drops of a concentrated sterile glucose solution are added and the cultures incubated at 37° C. for another day. Positive results in both sets of cases are then subcultured and classified.

It must, however, be stated that in the warmer months of the year the addition of the glucose may greatly increase the work without a correspondingly appreciable gain.

So far as the river water temperature is concerned, the year may be usefully divided into a period of five colder months (November, December, January, February, and March) and seven warmer months (April, May, June, July, August, September, and October), the water temperature during these two periods being on the average about 41° F. and 58° F. respectively. As a sort of compromise it might be desirable, as previously suggested, to use lactose

TABLE V.—*B. Coli* (*Lactose* +, *Indol* +).

Positive Results.	Raw Water.		Stored Water.		Filtered Water.		Alphabetical Classification.
	Actual.	Per Cent.	Actual.	Per Cent.	Actual.	Per Cent.	
1, 2, 4, 5, 10	23	13.53	22	20.56	21	27.27	A
1, 2, 4, 10	20	11.76	10	9.35	8	10.39	B
1, 2, 4, 6, 7, 10	1	0.59	—	—	—	—	C
1, 2, 3, 4, 6, 10	10	5.88	11	10.28	1	1.30	D
1, 2, 10	18	10.59	17	15.89	10	13.00	E
1, 2, 6, 10	3	1.76	7	6.54	—	—	F
1, 2, 3, 4, 5, 6, 10	10	5.88	13	12.15	10	13.00	G
1, 2, 5, 7, 10	12	7.06	8	7.48	3	3.90	H
1, 2, 3, 6, 10	9	5.29	2	1.87	2	2.60	I
1, 2, 3, 5, 6, 10	1	0.59	1	0.93	3	3.90	J
1, 2, 5, 10	28	16.47	9	8.41	11	14.29	K
1, 2, 7, 10	5	2.94	3	2.80	—	—	L
1, 2, 5, 6, 10	2	1.18	—	—	1	1.30	M
1, 2, 4, 6, 10	2	1.18	—	—	1	1.30	N
1, 2, 4, 5, 6, 10	2	1.18	2	1.87	2	2.60	O
1, 2, 3, 5, 6, 7, 8, 10	10	5.88	—	—	1	1.30	P
1, 2, 3, 4, 5, 6, 7, 8, 10, 11	2	1.18	—	—	1	1.30	Q
1, 2, 3, 4, 5, 6, 7, 8, 10	2	1.18	—	—	—	—	R
1, 2, 3, 6, 7, 10	1	0.59	—	—	—	—	S
1, 2, 3, 5, 6, 7, 10	4	2.35	—	—	—	—	T
1, 2, 4, 5, 7, 10	1	0.59	—	—	—	—	U
1, 2, 5, 6, 7, 10	1	0.59	—	—	1	1.30	V
1, 2, 3, 10	1	0.59	1	0.93	—	—	W
1, 2, 3, 4, 5, 6, 8, 10	1	0.59	—	—	—	—	X
1, 2, 3, 5, 10	1	0.59	—	—	—	—	Y
1, 2, 3, 4, 5, 10	—	—	1	0.93	1	1.30	Z
Totals	170		107		77		

1, 2, 3, 4, 5, 6, 7, 8, 9=gas in glucose, lactose, saccharose, dulcite, salicine, raffinose, adonite, inosite, and inulin respectively; 10=indol, 11=brown (V and P test). These microbes were not retested as regards their attributes. Had this been done, some of the smaller groups might have yielded different results and so have become absorbed into other groups. They are apparently not necessarily all real varieties of *B. coli*.

TABLE VI.—*100 Specimens of Coli-like Microbes Isolated from (a) Crude Sewage from various Sewage Works; (b) Various Effluents. B. coli (Lactose +, Indol +).*

+ = Gas

Group.	Cane Sugar.	Dulcite.	Raffinose.	Salicin.	A = Crude Sewage.		B = Effluents.	
					Actual.	Per Cent.	Actual.	Per Cent.
a	+	+	+	+	8	9.4	3	4.3
b	+	+	+	−	6	7.0	5	7.1
c	+	−	+	+	4	4.7	2	2.8
d	−	+	+	+	1	1.2	1	1.4
e	+	−	+	−	6	7.0		
f	−	+	−	+	12	14.1	11	15.7
g	+	+	−	−	1	1.2	4	5.7
h	−	+	−	−	5	5.9	12	17.1
i	−	−	−	+	26	30.6	15	21.4
j	−	−	−	−	16	18.8	8	11.4
k	−	−	+	+			3	4.3
l	−	−	+	−			3	4.3
m	+	+	−	+			2	2.8
n	−	+	+	−			1	1.4
Totals					85		70	

Fifteen specimens from A and 30 specimens from B were excluded from above table owing to their not being "lactose +, indol +" *B. coli*.

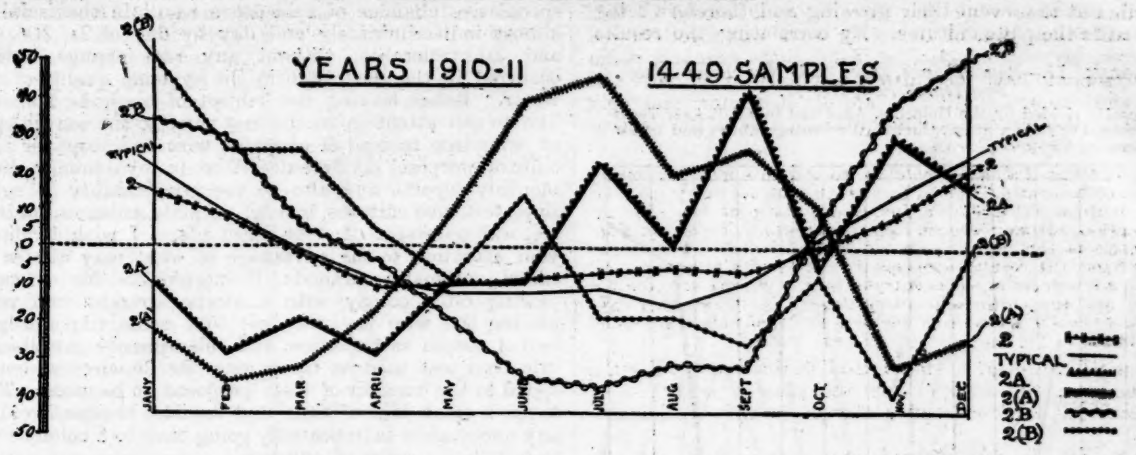
TABLE VII.

Out of 100 specimens of *B. coli*, isolated from 22 samples of human faeces, 88 per cent. were, as regards gas formation: Positive, that is, gas in lactose, glucose, mannite, maltose, galactose, laevulose, arabinose, sorbite, glycerin; and negative, that is, no gas in erythrite, amygdalin, glycol, inosite, inulin. These microbes all yielded indol and a negative result with Vosges and Proskauer's test. As regards adonite, cane sugar, dulcite, raffinose, and salicin, the following apparent varieties were obtained:

Group.	Adonite.	Cane Sugar.	Dulcite.	Raffinose.	Salicin.	Actual.	Per Cent.
α	+	+	+	+	+	2	2.3
β	+	+	−	+	+	3	3.4
γ	+	−	+	+	+	1	1.1
δ	+	−	−	+	+	22	25.0
ε	−	+	+	+	+	8	9.1
ϕ	−	−	+	+	+	10	11.4
ι	−	+	+	+	−	7	7.9
κ	−	+	+	−	+	1	1.1
λ	−	+	−	+	+	1	1.1
μ	+	+	−	−	+	1	1.1
ν	+	−	−	−	+	2	2.3
ο	−	−	+	+	−	9	10.2
π	−	−	−	+	+	10	11.4
ρ	−	+	−	+	−	1	1.1
σ	−	−	+	−	+	2	2.3
τ	−	−	−	+	−	1	1.1
υ	−	−	+	−	−	3	3.4
χ	−	−	−	+	−	2	2.3
ψ	+	−	−	−	−	1	1.1
ω	−	−	−	−	−	1	1.1
						88	

+ = Gas. − = No gas.

DIAGRAM I.—*B. Coli Test. Raw Waters.*



alone in primary medium during the warmer period, and during the colder period to add glucose at twenty-four hours should there be no gas formation within the first twenty to twenty-four hours.

Secondary Medium.—The value of neutral red bile salt peptone agar (rebipelagar) media in this connexion is indisputable. The Germans would seem to be very fond of Drigalski and Conradi's medium. Although, in my opinion, it is the best medium for streptococci, I do not think it is nearly as good for *coli* work as the bile salt media. At first sight it might seem almost ridiculous to raise any question as to the choice of a fermentative substance, as nearly all bacteriologists favour the use of lactose. Nevertheless, in my experience, as many "lactose +, indol +" *B. coli* are got by using saccharose (rebipesagar) as lactose (rebipelagar), and the chances are increased of isolating "cane-sugar negative" microbes. The best plan, in my experience, is to use both, and to subculture red colonies from the lactose bile salt medium and white colonies from the cane sugar bile salt medium.

Escherich's classical *B. coli communis* does not ferment cane sugar. Referring once more to my 1902-3 report on the normal stools of healthy persons, I find that the following results were obtained with cane sugar:

Only 8 of the 101 *B. coli* isolated yielded completely positive results; 35 gave quite negative results. Another 36 produced an acid change or a bleached appearance with which was associated either no gas formation or practically none; 20 gave rise to a bleached appearance with slight gas formation, and 2 showed a delayed and feeble power of fermenting the cane sugar.

As regard sewage and sewage effluents, the following results were obtained in respect to the fermentation of cane sugar:

	Sewages.	Sewage Effluents.
Positive results	29 per cent.	32 per cent.
Slight gas formation	3 ,,	5 ,,
Negative results	68 ,,	62 ,,
No record	—	1 ,,

* In another research, among 100 specimens of *B. coli* isolated from 22 samples of human faeces, 31 per cent. showed gas formation as regards cane sugar and the rest none.

On the whole I am inclined to think still, as I did in the past, that "lactose +, indol +, saccharose 0" microbes are more suggestive of recent human intestinal pollution than "lactose +, indol +, saccharose +" microbes.

Subcultural Tests.—Having isolated presumably *B. coli* organisms, there are two subsequent tests which I think ought always to be used—namely, lactose fermentation (ag = acid and gas) and indol production (in). In the old days I also used litmus milk (ac = acid clot) and neutral red broth (fl = fluorescence), and I spoke of "flaginac" microbes and their variations. The cane-sugar test was also applied to the flaginac microbes, and the symbols CSN and CSP used to express negative and positive results respectively. The litmus and neutral red broth tests nearly always give positive results with "lactose +, indol +" microbes. Any one, however, who has worked with cultures recently isolated from human faeces and waters of various degrees of purity must have been struck by the rapid clotting of the milk and strong fluorescence of neutral red broth with cultures made from faeces. These attributes are not always possessed in full degree by even "lactose +, indol +" microbes isolated from water. Weakness of these characters not infrequently helped me in judging the probable quality of the water supplies under investigation. For some considerable time now I have used gelatine media for indications of gas production, the reason being that I find solid media more delicate than liquid media for this purpose.

The fermentable substances used in a gelatine base are lactose, dulcite, cane sugar, and glucose, and of course the indol test is also habitually applied. The results are then classified as follows:

TABLE VIII.

Typical *B. coli* (Lactose +, Indol +)	2A, 2(A)	Saccharose +	Dulcite O, Dulcite +
	2B, 2(B)	Saccharose O	Dulcite O, Dulcite +

+ = Gas (or indol). O, No gas (or no indol).
1 = Glucose +, lactose O. 2 = Lactose +, indol O.

As previously indicated, cultures are made of 100, 10, and 1 c.cm., and lesser amounts of water, and the microbes showing the attributes grouped as 1, 2, 2A, 2(A), 2B, and 2(B) are classified according to their derivation from 100, 10, and 1 c.cm., etc. Sometimes, however, it is convenient to group the above varieties irrespective of the volume or dilution of water from which they were obtained, and to show the relationship of each one to the total number of specimens. I desire, therefore, to show you some curves in which this has been done, except that the "1 microbes" have been excluded from consideration. (See Diagram I.)

You will note that the results, when dealing with the raw Thames, Lee, and New River waters have been arranged as group-percentages each above and below its own mean. The curves representing the "2" and the "typical" groups (that is, all the "A" and "B" groups) are almost parallel, being highest in the winter months and lowest during the summer months. If the waters are actually most impure during the cold flood months of the year, then the "2" and the "typical" groups would seem to be better indices of quality than the separated "2(A)" and "2A" and "2(B)" group curves. On the other hand, the "2B" curve agrees with the "2" and the "typical" curves, but is even higher in winter and lower in summer than they are.

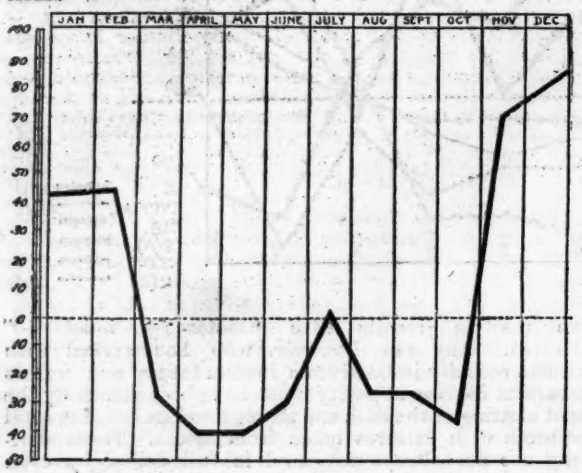
As illustrating the seasonal quality of the raw water, I show a diagram in which the results yielded by gelatine, agar, and bile-salt agar tests have been so arranged and combined as to give a single curve. (See Diagram II.)

The gelatine test tends to exaggerate the impurity of the water during winter, and perhaps to overestimate the improved quality of the water during summer. On the other hand, the bile-salt agar test sometimes yields high results during the summer months when the water, as judged by most other tests, appears to be better than the average. Possibly warm weather allows certain bacteria (probably not of excremental origin) to multiply in water,

and these same microbes are not sufficiently inhibited by the bile salt to prevent their growing and therefore being counted in the plate cultures. By correlating the results

DIAGRAM II.—*Raw Thames, Lee, and New River Water. Two-Year Period, 1910–11.*

Results yielded by the Gelatine, Agar and Bile-Salt Agar Tests combined to yield a Single Curve. (Percentage above and below Mean.)



yielded by the three tests, these differences are rounded off. It would, however, be begging the question to conclude that the curve thus obtained necessarily portrays the actual hygienic quality of the water. (See Diagram III.)

I take the opportunity of showing another diagram, illustrating the seasonal quality of the River Thames water over a five-year period. The results yielded by five chemical tests are shown both separately and in conjunction. (See Diagram IV.)

I now show another diagram dealing with the varieties of *B. coli* in water as supplied to consumers. Here the water, as supplied to the metropolis, has been dealt with in a similar fashion to that described under the raw sources of supply and the curves are more in agreement with each other, with the exception of the "2 (A)" curve, which is somewhat divergent. Excluding this one, it will be seen that the quality results are at their best about March, April, and May. Later there is a summer rise, which reaches its acme about August; then an autumn fall, finally followed by a marked rise in winter time. No doubt during the summer months the rate of filtration is increased, but it is a question whether the summer rise is not largely influenced by temperature. If this assumption be correct, the "2 B" curve, which tends to run highest in winter and lowest in summer, may really present the truest picture of the actual quality of the supply during the year. Another method of showing the *B. coli* results is to give the percentage number of samples yielding positive results in 100 c.cm. and less of water, as judged by the following three standards: "Lactose +"; "lactose +, indol +"; lactose +, indol +, saccharose 0." (See Diagram V.)

It will be seen that the three curves show a close relationship, but the "lactose +, indol +, saccharose 0" curve tends to run at a lower level during the summer months.

On the whole, I am inclined to think that the *B. coli* test is best regarded as a quantitative estimation of the number of "lactose +, indol +" microbes, and that the other tests should be regarded as supernumerary factors, enabling one to confirm or modify opinions principally based on the sheet-anchor lines referred to. What I am quite sure of is this, that with our London supplies the speculative chances of subculture result in the isolation almost indiscriminately and day by day of 2A, 2(A), 2B, and 2(B) microbes, without any real change having occurred in the meantime in the hygienic quality of the water. Before leaving the subject of methods I should like to call attention in the first place to the convenience of using iron instead of platinum wire and loops for subcultural purposes (as first described in my January, 1907, monthly report), and also to the practicability of using slope test tube cultures, instead of plate cultures, for subcultural purposes. In the third place, I wish to direct your attention to the advantage of what may almost be called automatic methods of inoculation—for example, picking off a colony with a sterile straight iron wire, placing the wire in a tiny test tube containing a drop or two of sterile salt solution, and subsequently introducing into that test tube as many more sterile wires as correspond to the number of tests proposed to be made. This saves a great deal of time and trouble, besides avoiding any uncertainty in repeatedly going back to a colony so as to inoculate a series of cultures.

SUMMARY OF CONCLUSIONS.

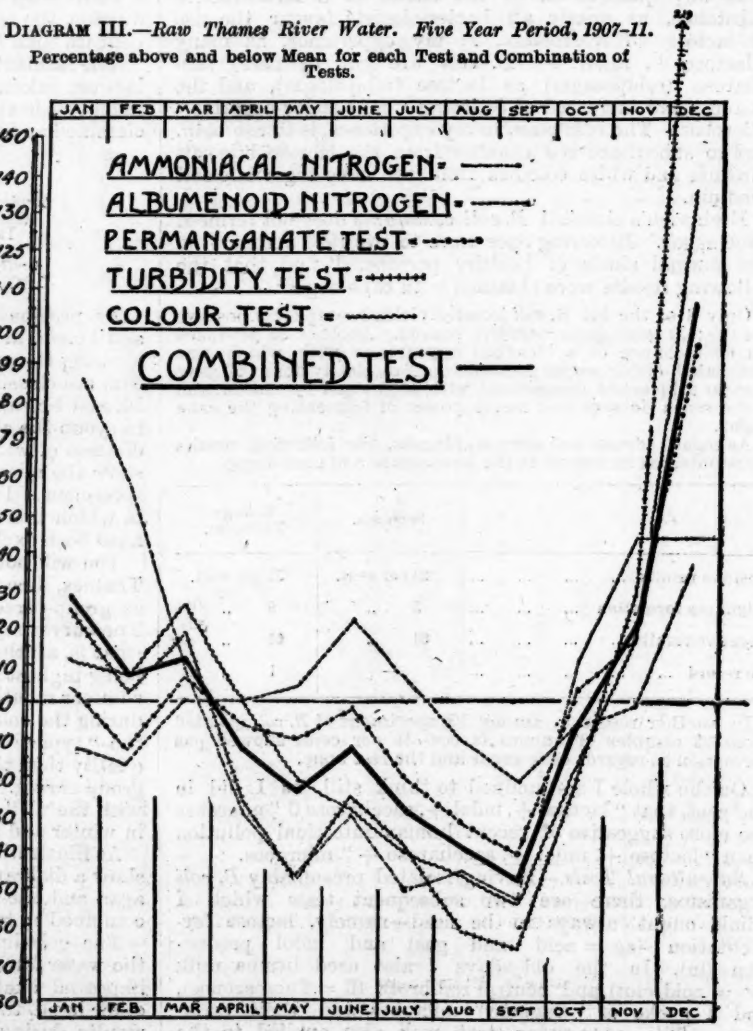
Typical *B. coli* (lactose +, indol +) is present in enormous numbers in excremental matters, and is absent from, or present only in small numbers in, substances free from undesirable pollution.

Typical *B. coli* is a decadent microbe when divorced from the animal body; hence its presence in a water in any number probably points to fairly recent pollution.

Pure waters, generally speaking, contain no typical *B. coli* in 100 c.cm. in a majority of representative samples; and incidentally it may be stated that impure waters can be so purified as to yield similar results at a not unreasonable cost.

DIAGRAM III.—*Raw Thames River Water. Five Year Period, 1907–11.*

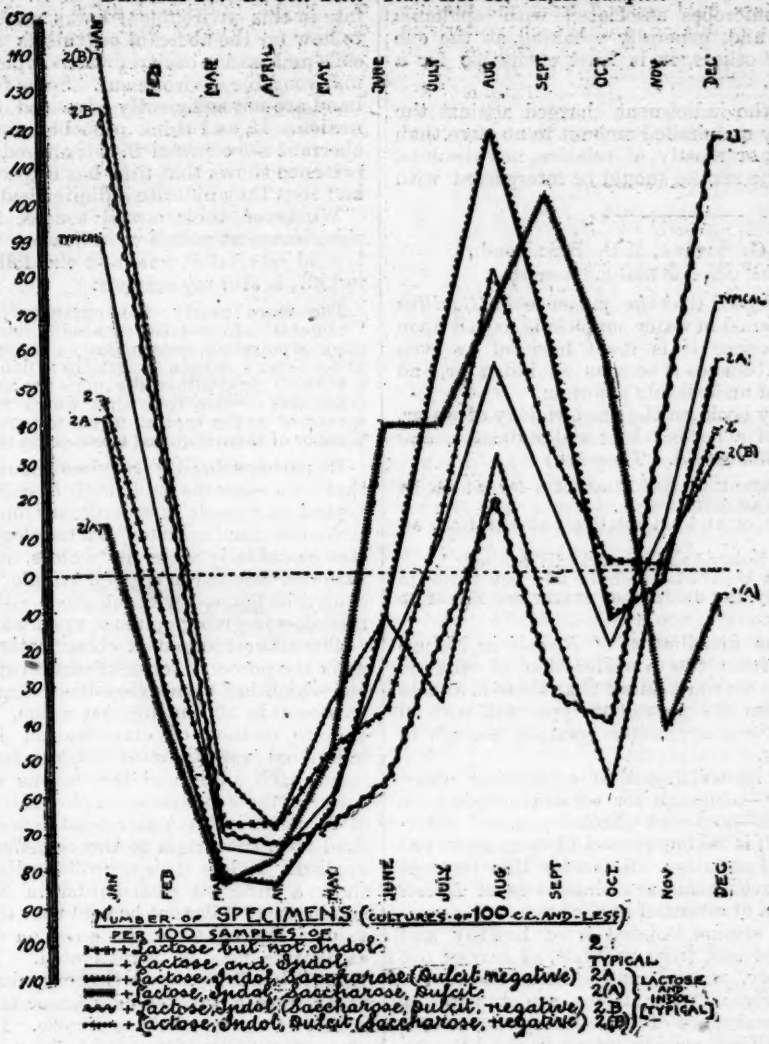
Percentage above and below Mean for each Test and Combination of Tests.



Even so lenient a standard as "no typical *B. coli* in 1 c.cm." of water in a majority of representative samples implies that the supply is not habitually contaminated with one gallon of sewage or its bacteriological equivalent, even in 100,000 gallons of water. The 10 c.cm. and 100 c.cm. standards (that is, no *B. coli* in these amounts) obviously connote the absence of $\frac{1}{10}$ and $\frac{1}{1000}$ gallon of sewage respectively from a like volume of water. With too severe standards there is danger of condemning reasonably safe supplies, and with too lax standards there is the possibility of passing waters which ought properly to be condemned. Always to steer safely between the Scylla of the one and the Charybdis of the other may be difficult or impossible without the aid of a pilot familiar with all the local conditions.

The *B. coli* test ought primarily to be regarded as a quantitative decimal enumeration of "lactose +, indol +" microbes, but the subsequent grouping, according to certain attributes of the organisms thus obtained, into fairly stable or apparent varieties, may be of real practical or diagnostic importance.

DIAGRAM IV.—*B. Coli Test. Years 1910–11. 14,149 Samples.*



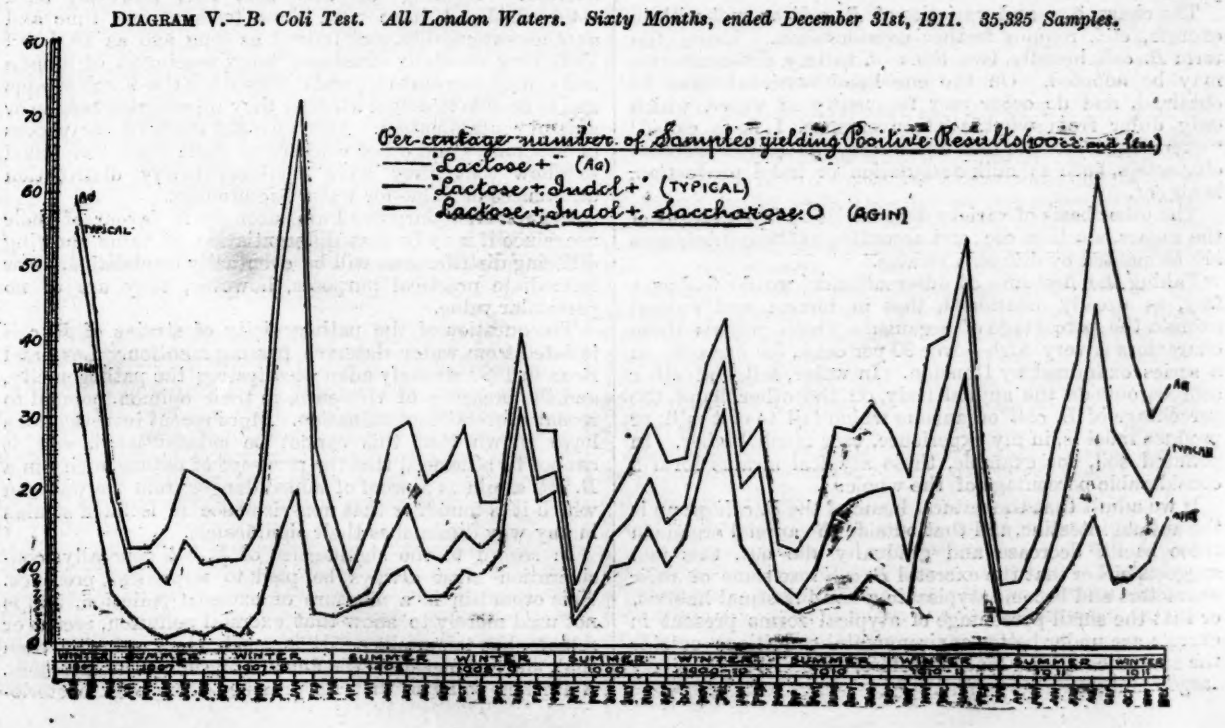
Standards are worse than useless if they are not interpreted with discretion and in relation to local and other conditions. Instances could be quoted where even similar *B. coli* results ought properly to lead to dissimilar conclusions as regards quality and safety.

The *B. coli* test even alone is of the greatest value, but a water supply should be finally judged on a summation of verdicts (geological, topographical, physical, bacteriological and chemical).

The *B. coli* test is by far the most reliable and speediest method of judging the degree of efficiency of the particular water purification process under investigation, and when a sterilization treatment is in operation, the certified destruction of *B. coli* should afford absolute proof, practically speaking, of the devitalization of all the microbes of epidemic water-borne disease.

The *B. coli* test having now survived the various vicissitudes of an earlier time, stands to-day as the most practical and delicate and rapid test for excremental filth, and may surely be taken as the most reliable indicator in its positive aspects of

DIAGRAM V.—*B. Coli Test. All London Waters. Sixty Months, ended December 31st, 1911. 35,325 Samples.*



possible danger, and in its negative aspects of the almost certain absence of microbes associated with epidemic water-borne disease, and, generally speaking, as the one test which, above all others, it is least excusable for a water analyst to omit.

All the counts in the indictment charged against the *B. coli* test when fully marshalled amount to no more than this: the test is only, or mostly, of relative, not absolute, value, and therefore the results should be interpreted with discretion.

II.—WILLIAM G. SAVAGE, M.D., B.Sc.Lond.,
County Medical Officer of Health, Somerset.

IT is now entirely accepted that the presence of *Bacillus coli* (and allied organisms) in water supplies is looked upon unfavourably, not because it is itself harmful or even possibly harmful, but because it acts as an indicator, and serves as a measure, of undesirable pollution.

I have set out in my book on the bacteriology of water, the four conditions of a perfect bacterial indicator, and these may be briefly mentioned. They are:

(*a*) It should be abundant in the substances for which its presence serves as an indicator.
(*b*) It should be absent, or at least relatively absent, from all other sources.
(*c*) It should be easily isolated and numerically estimated.
(*d*) Its characteristics should be definite and not liable to variation, whereby its distinctive characters might be impaired.

The question of the distribution of *B. coli* in Nature cannot be separated from the consideration of what is meant by *B. coli*, since we must admit that there is almost every gradation between the prevalent type met with in excreta and the coliform organisms common enough in harmless surroundings.

The object of the bacteriologist in examining water supplies is essentially—although for certain purposes he may have other objects—to detect whether excretal pollution (human or animal) is taking place and to measure the extent of such excretal pollution. Since for this purpose he uses *B. coli*, it is obvious that the characters of *B. coli* as found in excreta are of essential importance.

A study of *B. coli* strains isolated from healthy and diseased human beings and from animals, as carried out by Houston, McConkey, myself, and others, shows the following: That nearly all the strains are typical as regards the presence or absence of the ordinary characters; that no satisfactory differences can be made out between *B. coli* from healthy and from diseased persons; and that no differentiation can be established between *B. coli* from human and animal sources. These are all fundamental points.

The characters and varieties of *B. coli*, as met with in excreta, etc., require further consideration. Using the term *B. coli* broadly, two lines of variety differentiation may be adopted. On the one hand, varieties may be obtained, and do occur very frequently in water, which only differ from what, for convenience, I have called[1] "excretal" *B. coli*, in that one or other of the fundamental characters, such as milk coagulation or indol production, is absent.

The other basis of variety differentiation is by means of the sugars, alcohols, etc., and according as these substances are fermented by different strains.

Taking the first line of differentiation, we do find as a fact, as already mentioned, that in human and animal excreta the percentage of organisms which possess these characters is very high—over 90 per cent., for example, in a series examined by Houston. In water, soil, and other sources outside the animal body, on the other hand, the percentage of *B. coli* organisms which fail to clot milk or produce indol is, in my experience, very much higher. In polluted soil, for example, these atypical strains form a considerable percentage of the whole.

If we admit that the natural home of the *B. coli* group is the animal intestine, and that outside the animal organism these bacilli decrease and gradually die out, this fact suggests either that the excretal *B. coli* lose some of their characters and become atypical in a non-intestinal habitat, or that the small percentage of atypical forms present in excreta are under better environmental conditions outside the animal body, and tend, therefore, to persist in relatively large numbers.

In some experiments upon *B. coli* in soil[2] and their fate in this environment I was unable to satisfy myself as to how far the aberrant organisms met with in different soils and waters ever represent typical *B. coli* altered by unfavourable environment. Some further experiments in hand are not sufficiently advanced to quote in this connexion. If, as I think probable, the types only slightly aberrant are excretal *B. coli* altered by environment, their presence shows that time has elapsed since contamination, and thus they indicate a diminished danger.

Whatever their actual source, it is clear that their significance as pollution indicators is less than that of the typical *excretal B. coli*, and the following view, expressed in 1907, is still my opinion:

> The more nearly the organism isolated resembles an "excretal" *B. coli* the greater is its significance as an indicator of pollution, consequently the fewer the number required to condemn a sample of water in which they occur. Stated as a working proposition, the more the characters of the *coli*-like organisms deviate from that which for convenience may be spoken of as the typical form, the greater the proportionate number of them required to condemn the water.

It perhaps hardly requires mention, but the attitude that only concerns itself with *B. coli* which are completely typical as regards these ordinary characters is an impossible one for a water bacteriologist. These aberrant strains actually occur in waters, and may be the *only* varieties isolated in a given sample, and their significance cannot be ignored without great risk of making very bad mistakes in giving opinions upon water samples.

The other method of classification, based in the main upon the power to ferment certain sugars, alcohols, etc., is one which has been extensively employed, and for certain purposes is of the highest value. It is a precise and a definite method of classification leaving no room for individual variations of interpretation. On the other hand, each additional test means additional work. The value of their extensive employment for the differentiation of *B. coli* found in water must depend upon the light they shed upon the origin of the organisms to which they are applied. Unless their use differentiates organisms so as to show a different distribution in Nature and a different significance, it cannot be said that they are of any special value. For example, the question of the fermentation of saccharose may be investigated. To make this test of value it has to be shown that organisms which ferment this sugar have a greater or a lesser significance than those which do not ferment saccharose. If all *B. coli* in excreta or even a preponderance of them fermented saccharose, then the presence of a *B. coli* which was without this property would be of lessened significance, or might, for example, mean that it was not recently derived from excreta. Nothing of this sort has, however, been established. I have used these tests at one time and another extensively, and indeed, as long ago as 1904 and 1905, very carefully examined many specimens of human and animal excreta to try and differentiate the *B. coli* strains and so be able to detect whether they represented human or animal contamination. Also a careful study of the papers of McConkey and other workers on these lines has failed to show that they have established any distribution differences of value for water bacteriology.

For *research* purposes I am strongly in favour of their use, since it may be that differentiation of value showing differing distributions will be eventually established. For immediate practical purposes, however, they are of no particular value.

The question of the pathogenicity of strains of *B. coli* isolated from water deserves passing mention. Levy and Brun in 1899 strongly advocated testing the pathogenicity, and the presence of virulence, in their opinion, pointed to recent excretal contamination. More recent investigations have shown that this cannot be substantiated, and it cannot be advanced that the presence of pathogenicity in a *B. coli* strain is a proof of added danger from the water in which it is found, or that non-virulence in isolated strains in any way diminishes their significance.

In regard to the significance of *B. coli* generally, consideration must always be paid to numerical presence. This organism is a measure of excretal pollution, and is not used merely to show that excretal pollution, recent or remote, has taken place. It is much more valuable than that, since it measures the amount of such contamination. A further point is that its presence—like most bacterio-

logical tests—is of much greater significance than its absence. The one drawback of bacteriological examinations of water supplies is that they only indicate existing conditions; they do not indicate liability to contamination which is not actually taking place.

For example, a recent bacteriological analysis of a well water supplying five houses gave the following results: Organisms growing at 37° C. and 21° C = 2 and 15 per c.cm. respectively. No *B. coli* or streptococci in 50 c.cm.; larger amounts not examined. This well was a shallow one, sunk in porous ground, and surrounded up to a few yards by heavily manured gardens. It was on inspection obviously liable to pollution, while a chemical analysis disclosed a very high nitrate content (2.0 per 100,000), but low ammonia figures.

It is quite clear, as the very good bacteriological analysis showed, that the soil was acting as an efficient filter at the time the samples were collected. The bacteriological data did not, however, disclose that the good quality of the water depended upon an efficient soil filtration, and that this might break down at any time and without warning.

In conclusion, the particulars as to the following supply and its examination will serve as a good instance of the value of *B. coli* determinations:

The water is an important public supply, which supplies a large population. The water is derived from the mountain limestone. The limestone is extensively fissured, and careful topographical inquiry discloses that the supply is probably liable to contamination through these fissures from some of the villages and houses which are upon the gathering area. Apart from bacteriological examination, it is not possible to point to any definite specific pollution which is occurring. The following bacteriological analyses are of interest:

Date.	Number of Organisms per c.cm.		*B. Coli* (Excretal) per Litre.	Streptococci per Litre.
	37° C.	21° C.		
Oct. 4th, 1911 ...	200	1,000	30–100: 100–1,000 (atypical)	Absent in 50 c.cm.
Oct. 24th, 1911	100	500	1,000–10,000	1,000–10,000
Nov. 13th, 1911	120	950	1,000–10,000	1,000–10,000
Nov. 29th, 1911	60	—	30–100	Absent in 50 c.cm.
Jan. 16th, 1912	40	100	100–1,000	Absent in 50 c.cm.
April 29th, 1912	2	46	Absent in 100 c.cm.	—
June 4th, 1912	150	605	Over 10,000	Over 10,000
June 11th, 1912	37	145	1,000–10,000	100–1,000

This supply has been chemically examined upon a number of occasions, in every case without disclosing the slightest evidence of contamination. Incidentally they show the futility of chemical analysis to detect intermittent contamination of this kind.

The supply is derived from water running as an underground river which itself is evidently very pure, but which is liable to contamination from soakage from villages and towns on the gathering area, the contamination being shown after heavy rain following a previous drought.

The variation in the *B. coli* content is very striking.

REFERENCES.
[1] *Lancet*, February 4th, 1905. [2] *Journal of Hygiene*, 1907, vii, p. 477.

III.—W. WRIGHT, M.D., D.P.H.Cantab.,

Senior Assistant Medical Officer of Health, City of Glasgow.

IT is specially in regard to the possible causation of enteric fever that the presence of *B. coli* and its varieties in a water supply bears significance. There are some who attach considerable importance to its presence even in small numbers, and also those who would be disposed to disregard their existence unless the number of them was relatively large.

It was my intention to contrast the enteric attack-rates of the two cities, Glasgow and Manchester (whose water supply is unfiltered) and Liverpool and Birmingham (which is filtered) with their coliform content, after deducting those cases where the source of infection was known, with a view to ascertaining whether they bore any relation to each other. But, as such data are not available, I am reluctantly compelled to proceed on other more general lines.

Taking the two cities, then, whose supply is unfiltered—that is, Glasgow and Manchester—the following table has been prepared to show the contrast in enteric fever for the four years 1907–1910:

	Manchester.		Glasgow.	
	Cases Notified.	Attack-Rate per 1,000.	Cases Notified.	Attack-Rate per 1,000.
1907	265	0.41	470	0.58
1908	393	0.60	594	0.74
1909	369	0.61	566	0.70
1910	358	0.53	340	0.42

From this it will be seen that Glasgow occupied a worse position than Manchester during the years 1907–1909, but had a smaller attack-rate during 1910—that is, 0.42 against 0.53 per 1,000. Those figures are interesting enough in their own way as showing the incidence of the disease in the respective communities, but they are of no interest as they stand to the bacteriologist. It would be necessary to exclude from them all known sources of infection, such as cases imported or having derived their infection without the city. Further, all secondary cases—that is, cases obtaining their infection from previous cases in the same household, and such cases where there is reason to believe that infection has arisen from the consumption of shellfish, watercress, ice cream, or through the medium of a "carrier" or missed cases—should be eliminated in order to present a reasonable comparison of the effect, if any, of the water supply of one city as against the other. I do not seek to go into this apparently large divergence between the number of cases occurring in Glasgow and Manchester during the years 1907–1910 further than to mention the following facts:

During 1907–8 Glasgow suffered from a serious outbreak of enteric fever through contaminated milk, accounting for no less than 92 cases.

During 1909, 94 cases may be deducted, 17 of these being due to 2 missed cases, 21 to a carrier, and 56 to infected milk. Deleting these cases, the remainder furnish an attack-rate fairly comparable for the two cities. I am unable, however, to contrast the similar figures for the two cities during the whole period. I have chosen, therefore, the year 1910 in the case of Manchester, the last year for which statistics are available, and 1911 in the case of Glasgow, and I find that in the former city, of the 350 cases recorded, 59 are stated to be due to infected ice cream, 71 furnish a history of having recently consumed shellfish, and 62 to direct infection—in all 192—leaving 166, or 46 per cent., still to be accounted for.

In 1911 384 cases occurred in Glasgow, of which 100 fall to be deducted, 8 being imported, 78 secondary, 9 milk infection, and 5 shellfish, leaving a balance of 284, or almost 74 per cent., for which no source has been discovered.

It would have been interesting if it had been possible to deduct those cases where the source of infection had been ascertained, and thus contrast for the whole period the monthly curve of cases with the coliform content of their respective water supplies. But as the cases cannot be separated out in regard to their dates of onset, and as the coliform content of Manchester water is not available, this is not possible. The foregoing information is interesting, however, in this respect—that, despite the energies displayed by an inspector devoting his whole time to tracing out all possible sources of the disease, the Medical Officer of Health of Manchester has to acknowledge defeat in 46 per cent., whereas Glasgow concedes 74 per cent. Dr. Niven, in concluding this part of his report, says:

Apart altogether from carrier cases, there is some undiscovered mode or modes of propagation of this disease.

This is an expression which, one has not the slightest hesitation in saying, is upheld by every medical officer of health in the country. The question then arises, To what are the remainder of those cases due? What is the source of infection? As you will observe, the very best case has been made out in favour of some of those so-called known sources. In point of fact one has been forced to grant a considerable amount of leniency in accepting some of the sources mentioned; but even conceding those, whence

does the remainder, for which there is no explanation, obtain their infection? And when one remembers that those are isolated and invariably single cases in households, between which it is impossible to establish any connexion, it becomes all the more difficult to explain their origin.

I have compared in a general way the number of cases of enteric fever occurring in Glasgow during the years 1909–10–11 with the coliform content of Loch Katrine water. The actual figures are:

566 cases in 1909
340 ,, ,, 1910
384 ,, ,, 1911

By deducting those of known origin, excluding secondary cases, the figures so corrected are as follows:

470 cases in 1909
315 ,, ,, 1910
362 ,, ,, 1911

It will be observed that there is a considerable fall in 1910 as compared with 1909 and 1911. The coliform content of Loch Katrine water for the same period, however, affords no explanation of this. In 1909 coliform organisms, although found in 10 c.cm. practically throughout the year, were only found in 5 c.cm. during February, March, May, September, and October. In 1910 they were present in 5 c.cm. during nine months, and in 1 c.cm. in July, August, September, November, and December. In 1911 they were present in 1 c.cm. throughout the whole year, excepting the month of March. Those figures suggest, therefore, a steadily increasing contamination of Loch Katrine water without, however, a corresponding rise in the incidence of enteric fever in the city.

Similarly in the case of Liverpool, although the number of cases have fallen from 447 in 1908 to 231 in 1910, the coliform content of its water supply has, like Glasgow's during the same period, shown a steadily increasing rise. It is stated in the Bacteriologist's Report that in 1908 the colon bacillus was only found in a few samples of Liverpool water during July in 100 c.cm., in small quantities (unstated) during June and July, 1909, and from numerous observations (times unstated) in 1910 in quantities varying from 50 down to 10 c.cm. Whether this increase in coliform organisms in Glasgow and Liverpool water is due or not to improved bacteriological technique—and one has reason to believe that it is—it is clear it bears no relation to the incidence of enteric fever in either of these cities.

Coliform organisms have been present since June, 1911, to June, 1912, in 1 c.cm. of Glasgow's water every month, except March and June, 1912, in samples taken at the laboratory tap. Loch Katrine—the source of Glasgow's water supply—is stocked biennially with trout, and gulls abound on its water throughout the year. This is reckoned to be the source of *B. coli* and its varieties in the Glasgow water supply, for we are assured that no possible contamination of human origin can take place along the site of the supply pipes with water supplied at a pressure of 30 lb. to the square inch. If, then, *B. coli* and coliform organisms usually found in more or less quantity in water supplies generally are only of animal and not human origin, whence lies the significance of their presence in water as consumed by the public?

Whilst one acknowledges that the source of infection of from 50 to 70 per cent. of the cases of enteric fever throughout this country generally is not ascertained, and that therefore other influences towards its cause than those at present known to us are in operation, water contaminated even with *B. typhosus* would not explain those cases. Infected water, like infected milk, usually causes outbreaks of considerable magnitude, and should not be difficult to detect from the phenomena resulting. Moreover, apart from infected milk, enteric fever is recognized almost entirely as a disease of the artisan or poorer classes. There is no doubt whatever that the removal of decomposable filth by the substitution of waterclosets for middens and pail closets has done much to reduce the incidence of this disease in those quarters.

Dr. Robertson, the Medical Officer of Health of Birmingham, states in his annual report for 1910 that 73 cases of enteric fever occurred in that city during that year. Five of those were imported, or derived their infection from places outside the city; 6 were institutional, 10 secondary, and 19 furnished a history of having recently consumed shellfish, leaving 33 cases still to be accounted for. He adds:

> In 50 per cent. of the cases some reasonable explanation could be given as to the source of infection, while in the other 50 per cent. the source of infection remains quite unknown.

The number of pan privies in Birmingham in 1907 totalled 12,100. In 1910 those were reduced to 5,509. Two hundred and forty-eight cases of enteric fever occurred in Birmingham in 1907, or an attack-rate of 0.45 per 1,000. In 1910 only 73 cases occurred, or an attack-rate of 0.13 per 1,000. If Birmingham, by this means, can reduce the incidence of enteric fever to such an extent, there should be good reason to prophesy that when the remaining 5,509 pan privies are removed enteric fever should fall to an almost negligible amount altogether.

Those figures would appear to present eloquent testimony of the importance of removing decomposable filth from the precincts of dwellings; but when we contrast them with those of Glasgow, which for several years has possessed very few pan privies, and only 80 in 1911, we are forced again to conclude that other agencies are in operation.

The number of cases of enteric fever in Glasgow during 1910 was 340, or an attack-rate of 0.42 per 1,000, exactly four times that of Birmingham, which has 5,509 privies, against only 80 in Glasgow. The explanation, with but little doubt, however, is to be found in the different housing conditions existing in the two cities. The density per acre, and consequently closer association of the people, afford more ready means of infection in the one city as compared with the other. Birmingham, being more built on the separate house plan, as compared with Glasgow's tenements, has a density of 39 persons, against Glasgow of 60 persons per acre.

An unmistakable attack of enteric fever confers a certain degree of immunity, although instances of two or even more attacks in the same individual are not unknown. It is recognized also that certain mild forms of intestinal disturbances, which are in reality light but unrecognized cases of enteric fever, afford a certain protection against the severer forms of the disease. The occurrence, for instance, of two or more members of one family sickening at the same time or within a few days of each other suggest an earlier or missed case. We in Glasgow have had from time to time, as doubtless similar examples have occurred elsewhere, small localized outbreaks due to this cause, the initial case yielding a history far from satisfactory, although the nature of the illness was confirmed by the blood yielding a positive reaction to the Widal test, with later the successful isolation of the *Bacillus typhosus* from the urine or faeces or both. On the other hand, however, although satisfied that a "missed" case has been the source of infection of such cases sickening in common, one is not always able to prove this either by the history of previous illnesses in the family, their relations or neighbours, or by bacteriological examination. In seeking explanation of this, would one be justified in assuming that a relative freedom from enteric fever is enjoyed in some instances by permanent residents in a city having what bacteriologists might term a slightly impure water supply, as compared with the susceptibility of the stranger within the gates?

For example, during the South African war our troops suffered very severely from enteric fever. I personally witnessed the suddenness of that outbreak shortly after the occupation of Bloemfontein, when several hundreds were laid low in the course of a few weeks. It was assigned, I believe, to the contamination of the water whilst our troops were holding General Cronje at bay before his surrender. Was that outbreak started by gross contamination of the water by *B. coli* only, or to *B. typhosus* from one or more unrecognized cases of enteric fever, or to "carriers"? The *Bacillus typhosus*, we are reminded, however, breeds true, and typhoid only arises from typhoid, recognized or unrecognized, in the form of a carrier. Whilst our troops suffered so severely at that time and later, how is one to explain the great immunity enjoyed by the Boers in this respect, not only then but throughout the whole history of the war? Is the explanation to be found in that they all suffered at one time in their life-history from severe, mild, or unrecognized enteric fever? Such an attitude is indefensible. Or was it that, throughout the course of years, maybe, the

general unsatisfactory condition of their water supplies (and this applies equally to a fair number of our country districts) was, by some means still unknown to us, producing this effect?

Some time ago I had occasion to visit a farm in one of the counties adjoining Glasgow on account of the occurrence of enteric fever on a particular milk supply, and in the course of my examination of that farm I learnt that the sewage from the byre found its way into a burn which was the sole source of water supply of a village 60 to 80 feet down the hill, where some twelve to fourteen families resided. This water was supposed to pass through a crude sand filter before being supplied to the village, but this filter had long been neglected, with the result that the water flowed freely over the top.

On making inquiry, I learnt that not a single case of enteric fever was known to have occurred in that village for many years past. Crude sewage of animal origin in bulk was thus finding access into that burn daily throughout all these years, and not a single case of enteric fever resulted. Had the bacteriologist examined that water, hundreds of *B. coli* would have been found, without doubt, in every cubic centimetre, yet the people not only thrived, but were the picture of health.

Those of you who have had experience of county work must agree that it is sometimes astonishing—looking at the subject from the bacteriologist's point of view—how the inhabitants of some districts have escaped. This instance is given only to emphasize the necessity of placing the significance of *B. coli* and coliform organisms in a water supply on a true basis, for until we can be authoritatively informed that a particular water supply is contaminated with organisms of human origin, indicating thereby the possibility of infection from a recognized or unrecognized case of enteric fever, or carrier, we may be excused in affirming that the presence of *B. coli* and its varieties in a water supply is not of great significance after all.

DISCUSSION.

Dr. M. J. OLIVER (M.O.H., Roxburgh) pointed out that the kinetics of water in motion were not the same as those of stationary water. Owing to partial obstructions in the main, due to bends or irregularities of joining, there might frequently be *minus* pressure at certain points, causing insuction of liquids through minute apertures. Again, many parts of such mains were above the hydraulic mean gradient, and the pressure might vary from *plus* to *minus*, according to the velocity of the flow. With regard to Dr. Wright's statement that it was remarkable that in a village supplied with polluted water no cases of typhoid fever had occurred, he directed attention to the fact that in such small populations it might require many years for results to show themselves in this manner. Taking a typhoid mortality of 0.1 per 1,000 persons per annum, or 0.01 per cent., this would work out at 1 case of enteric fever per 100 persons in one hundred years. In the speaker's experience, the local remembrance of cases of fever seldom exceeded twenty years or thereabouts.

Dr. W. G. WILLOUGHBY (M.O.H., Eastbourne) thought a handy guide to the safety of a water supply was greatly needed, and he asked Dr. Houston whether the total absence of *B. coli* in 100 c.cm. of water was evidence that such water was quite safe. Was it possible that storage, etc., could cause a disappearance of *B. coli* but not of pathogenic germs? The diagrams showed that there were apparently about nineteen varieties of lactose +, indol + *B. coli*; was it safe to condemn or classify water supplies on the presence of one of these nineteen varieties in 10 c.cm. of water until it could be shown that they were all equally good indicators of pollution? As far as the speaker was aware, no tables or figures existed which revealed the presence of diarrhoeal diseases in any district in relation to the presence of *B. coli* in high proportions in the water supplies of those districts. He was satisfied that at present the *B. coli* test acted as a safe indicator in conjunction with other forms of examination, such as topographical and chemical investigation.

Dr. C. O. STALLYBRASS (Assistant M.O.H., Liverpool Port Authority) said: A high relationship has been shown in the United States between sewage-polluted water supplies and the incidence of diarrhoea, which is parallel to that of enteric fever in cities with such supplies. In Liverpool the cases of enteric fever which could not be accounted for in the manner in which Dr. Wright referred to amounted to about 50 per cent. in 1911. But these cases were unequally distributed throughout the city. Distributing these cases into an outer sanitary area and an inner area characterized in the past by a large number of courts of an insanitary character, the incidence of these unaccounted-for cases was about five times as heavy in the inner area as in the outer, showing that a large proportion of them cannot be attributed to the water supply, but are rather to be accounted for by the housing conditions. This inference is borne out by the high correlation between the great reduction in cases of typhoid fever in Liverpool and the corresponding destruction of courts in the central area and their replacement by sanitary dwellings. The characteristic of the court houses was the presence of common trough closets. Infantile diarrhoea persists in those areas where typhoid used to occur in the past.

Dr. W. J. PENFOLD (Lister Institute) said that in faeces and urine non-lactose fermenting organisms were frequent, which fermented lactose after a short selection on a lactose medium. The non-lactose fermenters not infrequently appeared to be closely related to *B. coli* (Escherich), while at other times they were not so related. It seemed, therefore, desirable to examine the nature of those organisms which did not cause lactose fermentation immediately, in order to determine the closeness of their relationship to true *B. coli*. A consideration of McConkey's work would tend to suggest that *B. vesiculosus* was relatively characteristic of human faeces, and that consequently the use of an extended series of sugar tests should be of practical value.

Dr. RICHARD JONES (M.O.H., Merionethshire), speaking of the practical difficulties of country medical officers of health, said the population of his district was 12,000. In a report of the Local Government Board the number of deaths from typhoid fever in the district during the ten years 1868–78 was stated to be 12 per annum, 120 in all. The present water supply was obtained in 1879. During the following ten years the deaths from enteric fever fell to 1.0 per annum. The supply was from a natural mountain lake, 1,300 ft. above the sea level, with no possibility of contamination from human sources. There had been no enteric fever in the district during the past eighteen years. The authorities, wishing to improve the supply, and particularly to remove the peaty matter, asked his advice as to the best method. Prior to giving this the speaker had the water examined by bacteriological and other methods. Alien *B. coli* were found to be present in 1 c.cm., and in one sample in 0.1 c.cm. of the water. There had been no enteric fever in the district for eighteen years, nor any other disease which might reasonably have been attributed to the presence of *B. coli* in the water. A sum of about £3,000 would be expended. A Local Government Board inquiry had been held, and the inspector had taken a serious view of the presence of *B. coli* in the water. Dr. Houston had said that no means existed of distinguishing between *B. coli* of animals and of human beings. The lake was visited by swarms of geese during the summer months. He raised the question whether the *B. coli* of birds such as geese could be distinguished from those of the human being. After what had been said in the discussion, he would hesitate to advise a large expenditure on purification to remove *B. coli* in an otherwise pure supply when no enteric fever had existed in the district.

Professor JAMES RITCHIE (Edinburgh) considered that the most important point in Dr. Houston's paper was his insistence on the necessity of the results of the bacteriological examination of water being considered along with the results of a personal inspection of the locality. He hoped that the effect of the discussion would be that no bacteriologist would consent to give an opinion on the safety of a water without having the opportunity of considering its natural surroundings, and that no medical officer of health would accept a bacteriological report unless this condition had been complied with in framing

it. With regard to the more strictly bacteriological part of Dr. Houston's paper, he thought that bacteriologists were by no means clear as to the characters to be looked on as those of typical *B. coli*. This was especially the case when the development of indol was made a type character. In many waters organisms were found, of undoubted excremental origin, which failed to give a positive indol test. One of Dr. Houston's curves showed that over a long period in London waters the number of simple lactose fermenters varied directly with the number of bacteria which, besides fermenting lactose, produced indol. This bore out the view that atypical forms of *B. coli* in water did really come from excremental sources. Much investigation was still necessary regarding the effect of growth in artificial media on the permanence of fermentative reactions. Recent work on streptococci along parallel lines accentuated the necessity for adopting an attitude of caution before assuming that all the apparently different varities of the organism based on the fermentative reactions were in reality different. In certain waters too little attention had been paid to the serious pollution effected by gulls which had shortly before been feeding on street refuse.

Dr. A. K. Chalmers (M.O.H., Glasgow) said that, great as had been the reduction in the death-rate from enteric fever during the past generation, there were many apparent anomalies which required explanation. He had little belief in an irreducible minimum with regard to infection or disease. The minimum to be aimed at was its absolute prevention. Notwithstanding the reduction in the death-rate, the contrast between many places equally well administered required to be explained. For example, the death-rate in Scotland in 1900–2 was slightly below that of England (142 per million against 152), but the proportion of Scotland's population at ages between 25 and 35, when enteric fever was most fatal, was lower than in England. Or, again, it might be said that the West of Scotland suffered more heavily from enteric fever than the East. The country districts in Scotland, on an average, had a slightly greater incidence of the disease than the principal towns, whilst it was heaviest in the smaller burghs. Then, again, in what might be called the agricultural counties, the death-rate in landward districts was on the average below that of the burghs in these counties; whereas in the industrial counties, the coal and iron centres, the rate in the landward districts was higher than the average for the burghs. This was probably relative to the condition of the numerous groups of population scattered throughout these districts engaged in mining and other industries. In any case, the differences in the incidence which these several areas presented were of themselves sufficient to demand inquiry and the endeavour—which the discussion would, he felt sure, make clearer—to inquire whether these differences might be associated with differing degrees of bacterial content of water, was a new departure and one likely to yield help in the future researches on the disease.

Dr. Houston, in his reply, corroborated what Dr. Oliver had said in regard to insuction of liquids into an iron main. He instanced the finding of insects in the water supply in Edinburgh a few years ago. Dr. Williamson had been able to prove that they had been drawn into the mains. Replying to Dr. Willoughby, he stated that the absence of *B. coli* in 100 c.cm. of water might be accepted as evidence of the absence of pathogenic microbes, since the decadence in these was at least as great as that of *B. coli*. Typhoid bacilli disappeared in something like nine days from water as a rule. His experiments seemed to show that the death of *B. typhosus* could be concluded from the ascertained death of *B. coli*. Having expressed himself with caution in regard to the classification of the colon bacillus, according to the fermentation tests, he agreed with Dr. Penfold that further investigation of such microbes which changed their behaviour to lactose would be advantageous in each case, and explained the routine which he adopted at the Metropolitan Water Board. Dr. Richard Jones's case obviously could only be satisfactorily dealt with after local inspection had been carried out. He entered briefly into a discussion of the pollution of water by means of gulls after feeding on garbage, as compared with feeding on fish. In dealing with the question of the high incidence of pollution of London waters in winter time, and the remarks made by Dr. Chalmers relative to the Scottish conditions, he questioned whether a fully satisfactory explanation could be obtained on the basis of occupation and the like, and was inclined to think that the influx of storm water from over-filled burns played some part in raising the pollution.

Dr. Wright also replied to a few points.

It was then moved by Dr. Oliver, seconded by Professor Ritchie, and carried unanimously:

> That this Conjoint Meeting of the Sections of State Medicine and Bacteriology unanimously desires strongly to urge that no opinion as to the quality of a water for dietetic purposes should be arrived at on bacteriological evidence without a local and topographical inspection of the sources of the supply made by a competent observer.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

THE USE OF URANIUM AS A RADIO-ACTIVE THERAPEUTIC AGENT.

The results obtained by Czerny and Caan from the use of mesothorium in the treatment of skin diseases are of great interest, and my experiences during the last two years with the use of uranium in the same class of diseases may be of some interest in corroborating their claim on behalf of the former substance.

I have used a yellow oxide of uranium, prepared for me by Mr. J. C. Sharland, and the nitrate in solution.

After test treatment of several cases with varying strengths and different methods, I treated one of recurrent carcinoma of the breast, which was deemed inoperable. There was a tumour as large as the fist in the scar, with a sinus extending into the chest and extensive ulceration, also two separate nodules at the site of a stitch wound. There was frequent and profuse haemoptysis, and the patient was very exhausted.

I advised as a forlorn hope fortnightly injections of 15 grains of the oxide of uranium suspended in sterile oil into the substance of the growth. In a few months the tumour had practically disappeared, the haemoptysis had ceased, the nodules had gone, and the patient's general condition so improved that she was able to do her own housework. To accelerate the superficial healing I covered the ulcerated patches with silver foil, and gave weekly *x*-ray exposures of 2 H. with good results.

Of three similar cases, I obtained good results in two and no benefit in the third.

In a case of recurrent carcinoma of the parotid gland, I removed the growth as well as I could by the knife, and powdered the wound with the oxide of uranium. There has been no return up to now—a period of five months.

In two cases of rodent ulcer which were not affected by *x* rays I used ionization with 2 per cent. solution of the nitrate, with the result that healing commenced immediately.

In the treatment of lupus erythema I have obtained good results by the intramuscular injection of soamin (gr. v to viii) on alternate days, and the bi-weekly use of *x* rays from a tube having a penetration of 5 W. in doses of about 2 H. As an ointment in the same cases I am in the habit of using the following: ℞ Uranii oxidi ʒij, lanolinum ad ʒj. This sometimes causes quite a lively exacerbation of the erythema, but used about once in ten days is very useful. In psoriasis and senile atrophy of the skin I have found the ointment useful, and also a lotion of the nitrate containing 20 grains to the ounce of water.

Alfred Clark, F.R.C.S.Edin.,
Honorary Radiologist, Auckland Hospital, N.Z.

CASES OF EXOPHTHALMIC GOÎTRE MAINLY ASSOCIATED WITH THE PUERPERIUM.

There is reason to think that exophthalmic goitre is common in Leeds; I have met with very many cases. Several of these have recovered independently of treatment—in fact, long after treatment had been given up. There were also, as will readily be believed, a large number whose condition suggested hyperthyroidism, and in whom such symptoms as increased pulse-rate have been aggravated when thyroid extract has been given. This

positive reaction to the thyroid extract test appears to be of some importance. In two instances the exophthalmic symptoms drifted into mild myxoedema; mental instability was present in two more. Two other patients gave a history of fright, which apparently precipitated the attack.

The following cases are particularly interesting:

1. Mrs. A., aged 45; menstruation irregular; patient irritable, mildly excitable; tachycardia; slight tremors. She had lost a son with tragic suddenness from diphtheria. The exophthalmos became much worse soon after her daughter was attacked by pulmonary tuberculosis. The mother took this illness of her daughter greatly to heart; subsequently, indeed, she abruptly dismissed the medical man who had found tubercle bacilli in the daughter's sputum.

2. Mrs. B. showed the classical signs of exophthalmic goître when she was in bed slowly recovering from childbirth. The confinement had been very tedious and difficult. She had been both exhausted and, as she said, terrified. Instruments had been used, without chloroform; the child had died, and the mother had been very ill. There had been no sign of Graves's disease before the confinement. She lived for several years. Attacks of diarrhoea were common, and she was subject to frequent febrile conditions. She ultimately died of pulmonary tuberculosis, aggravated by mitral insufficiency. This case was also seen by Dr. Barrs of Leeds.

3. Miss C. gave birth to an illegitimate child. During her convalescence it was noticed that her eyes were protuberant and the thyroid enlarged. There was some tremor, but not much. Five years afterwards the signs and symptoms had mostly disappeared.

4 and 5. About the same time I saw two other cases of exophthalmic goître following close upon the puerperal state. In one of these cases the mother was unmarried.

6. Mrs. D. Sir Berkeley (then Mr.) Moynihan, who saw the case with me, found her suffering from a severe internal haemorrhage, due to tubal rupture. The case was proved by the pathologist to be one of tubal gestation. The patient had no signs of exophthalmos before the operation, as was shown by a photograph taken shortly before. The exophthalmic appearance and symptoms came on during the time the patient was in bed; they were very marked one month after operation. Five years after the appearance the patient was found to have glycosuria and polyuria. She has recently died from diabetic coma.

H. DE CARLE WOODCOCK, M.D.,
M.R.C.P., D.P.H.Edin.
Leeds.

Reports
ON
MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS AND ASYLUMS OF THE BRITISH EMPIRE.

COSSHAM HOSPITAL, BRISTOL.

RENAL CALCULI (OXALATE) WITH SEVERE HAEMORRHAGE AFTER NEPHROLITHOTOMY.

(Under the care of Dr. HEY GROVES.)

[Reported by W. A. REYNOLDS, M.B., Ch.B.Bristol, M.R.C.S., L.R.C.P., House-Surgeon.]

THE patient in the following case, a man aged 23, was admitted on December 27th, 1911. He stated that he had suffered for two years from continuous discomfort in the left side. There had also been occasional sharp pains in the side lasting about a minute, together with intermittent haematuria. There had been no increase in the frequency of micturition nor passing of gravel, so far as the patient was aware.

State on Admission.

His urine was straw-coloured and acid, the specific gravity 1025. There was a slight trace of albumen but no blood, no sugar. The total quantity passed in twenty-four hours was 28 oz. The lower pole of the left kidney was palpable, but not tender. A skiagram showed several renal calculi, one being the shape of a horse-shoe.

Operation.

On January 13th, 1912, the left kidney was exposed and delivered through the usual lumbar route. On palpating the organ several soft areas containing hard lumps could be felt. An incision was made into the largest of these, and the large horse shoe-shaped stone shown in the skiagram was removed, one limb of the "horse-shoe" being fixed in the orifice of the ureter. Clear fluid and blood, but no pus, flowed out of the wound. The incision into the kidney was elongated so as to open it up from its convex border. About a hundred stones were now removed with a scoop.

A good deal of blood was lost while they were being got out. A catheter was passed down the ureter and withdrawn, and then water at 110° F. was poured into the opened-up kidney, and the two halves were sewn together with deep catgut sutures, except that a rubber drainage-tube of medium size was left in, so as to drain from the interior of the pelvis of the kidney out to the surface of the body. The kidney was then replaced in the abdomen, and the incision closed except for the drainage tube, the muscles being sewn with catgut and the skin with silkworm gut.

Progress.

After the operation the patient was rather collapsed at first, pulse 126, respirations 42. He was given a saline infusion per rectum, 1 pint every three hours.

Next day he was better, pulse 112, respirations 30, and temperature 100.2° F. The dressing soaked through several times, chiefly with urine. The tube was removed on the 15th, and a gauze drain inserted. He was much better on the 16th, pulse being 86. He seemed to be going on well, and was talking to the men in the ward, saying how "fit" he felt in the early morning of the 17th.

Occurrence of Haemorrhage.

About 9 a.m. that day he complained of sudden pain in the left side, with pallor and pulse-rate of 132. On examining the wound a steady stream of blood was seen to be flowing out; the two stitches nearest the drain were removed, and the wound opened up, and the sinus down into the kidney was syringed with hydrogen peroxide, and then gauze soaked in adrenalin 1 in 1,000 was inserted. This treatment stopped the haemorrhage.

About an hour and a half later a rubber tube was passed down into the kidney, and 10 oz. of an adrenalin solution (adrenalin chloride 1 in 1,000, ʒj, to water ʒj) was poured down the tube to thoroughly irrigate the wound. Cold fluids only were given.

After this there was no further haemorrhage from the wound, but blood clot accumulated in the bladder, causing retention of urine, with the occasional involuntary passage of blood and clots by the urethra. This caused severe pain, and on January 20th an attempt to clear the bladder through a Bigelow's evacuator was made.

This having failed, nitrous oxide was administered, and a suprapubic opening was made into the bladder and the clots turned out. A tube was left in, and a Hamilton-Irving apparatus fitted over the wound. The patient on the termination of the proceedings was extremely pale and collapsed, pulse-rate being 140. He was put on calcium lactate, grains xv four-hourly, with plenty of hot fluids, and brandy ʒss three times a day.

Result.

He slowly improved without further haemorrhage from the wound, though blood was passed in the urine. Clots were washed out of the bladder through the suprapubic wound twice daily for several days. By the 25th no more clots were found in the bladder. During this time much urine but hardly any blood passed out through the lumbar incision, and 41 to 60 oz. were passed daily through the suprapubic wound, large quantities of fluid being taken by mouth. On January 28th the tube in the bladder was removed, together with the Hamilton-Irving apparatus, and the wound was packed with moss. He was given iron, and from this time made an uninterrupted recovery, urine passing through the urethra after two soft rubber catheters had been tied in for twenty-four hours each, at the end of a week from when the tube was left out; and he was discharged with both wounds practically healed on March 2nd, 1912.

The calculi were found to consist almost entirely of calcium oxalate, associated with a very small amount of amorphous organic matter. Uric acid was not detected. As already stated, they numbered about 100; the majority were of the size and shape of large shot, but several were shaped like Indian clubs, and one resembled the knob of a traditional giant's bludgeon.

Note by Dr. Hey Groves.

There are several points of interest in the above case. The number of the stones was very unusual; probably the large stone in the pelvis was the primary calculus which caused obstruction to the outflow of urine, and the consequent stagnation brought about the deposition of the numerous small stones. The bizarre shape of the medium-sized stones was very unusual.

From a practical point of view the question of chief interest is, At what point is a kidney to be regarded as so disorganized as to require removal? In a young patient with plenty of healthy kidney substance, nephrectomy is not justifiable, but nevertheless it would, of course, be a much easier operation than removal of the stones. The occurrence of such a severe haemorrhage from the kidney, with subsequent filling of the bladder with clot, is fortunately a very rare sequel to nephrolithotomy.

Reviews.

CEREBRAL DECOMPRESSION IN ORDINARY PRACTICE.

SOME years ago a distinguished authority is reported to have said that the treatment of intracranial tumours formed rather a melancholy chapter in therapeutics. How little this, so far as it is now true, is the fault of modern surgery is shown by Mr. BALLANCE'S recent summary[1] of the advances that have been made of late in the local diagnosis and treatment of abnormally raised intracranial pressure.

At the present time there are three methods of effecting cerebral decompression. By the first method, that of lumbar puncture, a moderate amount of the cerebro-spinal fluid is withdrawn from the subarachnoid space. By the second, continuous drainage of the fluid from one of the ventricles of the brain, either into another cavity—the arachnoid space—or externally, is effected through a small metal or glass tube. By the third, a part of the skull is trephined and removed, so as to make an opening for drainage or for the establishment of a hernia; such decompression is most effectual when made over the actual site of the cerebral disease, and so the accurate localization of the intracranial lesion becomes a matter of the utmost importance.

The intracranial lesion—whether a tumour, an abscess, a haemorrhage—acts as would a foreign body within the skull, and raises the intracranial pressure by causing the quantity of cerebro-spinal fluid to increase. Death is apt to ensue as a result of the general raising of the intracranial tension, and decompression by craniectomy averts the dangers of this general pressure. And so, when the general intracranial pressure has been thus lowered, an end is put to the most distressing symptoms that render many of these cases so painful to deal with—the vomiting, the intense headaches that resist all other treatment, the optic neuritis that proceeds to optic atrophy and blindness. In ordinary practice, cerebral decompression may be undertaken either for injury to the head or for disease within the skull. It is indicated in cases of extradural haemorrhage from rupture of the middle meningeal artery, or in patients with intradural haemorrhage such as may occur when the skull is fractured either at birth or in later life. In these cases the indications for immediate operation are often obvious. But there is a danger lest the remote effects of such injuries should fail to receive proper treatment in the patients who survive, and Mr. Ballance instances intermittent headache, intermittent paralysis, and intermittent insanity occurring after head injury, as symptoms suggesting subdural haematocele, a condition certainly remediable by operation. He quotes a number of lesions in which decompression for disease in the brain should be essayed perhaps more often than it is. Immediate operation in cases of cerebral haemorrhage might, it is suggested, often be attempted, craniectomy being performed and the brain incised for the escape of blood and clot behind the fissure of Rolando and above the Sylvian fissure; a strip of gauze soaked in adrenalin and passed through the incision in the brain would, it is maintained, suffice to arrest the haemorrhage. Decompression for hydrocephalus by continuous drainage of the cerebral ventricles through a cannula into the subdural space or the subcutaneous tissues has been carried out with success in many cases. In suppurative meningitis wonderful cures have followed decompressive operations. Mygind had six recoveries out of twenty-six operations for meningitis occurring as a complication of ear disease; and Tedesko has reported a case cured by lumbar puncture, where pus was drawn off by the exploring needle. In serous meningitis, including certain cases of heat apoplexy, status epilepticus, and acute delirious mania, craniectomy and incision of the oedematous cerebral substance would appear to offer a hope of recovery, should lumbar puncture fail.

But it is in cases of cerebral and cerebellar abscess or tumour that cerebral decompression seems to have proved most successful. Such operations have long been in current practice, and the importance of their early performance is recognized. Early diagnosis and early operation before irreparable damage to the eyes has occurred are the great desiderata here. The presence of optic neuritis demands immediate operation. To make an osteoplastic flap is a mistake; a cranial defect is essential, a cerebral protrusion is desirable. It seems to be settled that when the tumour can be localized, the craniectomy should be performed over its site; but in all infiltrating tumours, simple decompression over some silent area of the brain is the method of choice, when the free escape of cerebro-spinal fluid will rapidly relieve the congestion of the optic discs. Mr. Ballance reports many illustrative cases, and gives convincing proof of the efficacy of cerebral decompression in a large number of different cases.

[1] *Cerebral Decompression in Ordinary Practice.* By C. A. Ballance. London: Macmillan and Co. 1912. (Demy 8vo, pp. 71; figs. 52. 2s. 6d. net.)

TEXTBOOKS OF OPHTHALMOLOGY.

WE have received the first volume of a *Textbook of Ophthalmology*,[2] written on somewhat original lines by Dr. PAUL ROEMER, Professor of Ophthalmology at Greifswald, and translated into English by Dr. M. L. FOSTER of New York. Its originality consists in the fact that it is produced in the form of clinical lectures. In this volume nothing behind the lens is discussed, but a very thorough account is given of all external diseases of the eye, as well as that of the iris and lens. The anatomy is first described, then the methods of clinical examination, after that the diseases. An endeavour has been made to render the book attractive to students, and one very unusual feature about anything calling itself a textbook is this—that it is written in a colloquial style, and must admirably call to mind the cases and the surroundings of the clinic in which they were delivered. Considerable attention is bestowed upon diseases which owe their origin to bacteria, and several coloured plates are given which show different forms of organisms. A fairly thorough explanation will be found of serum-therapy and of the Wassermann reaction. The author strongly disapproves of boiling fine cutting instruments, which he states causes dullness of their edges. This is an opinion very commonly held by barbers with regard to razors. Professor Roemer recommends that knives should be placed for two hours before and two hours after the operation in absolute alcohol, and that all instruments should be used dry. Minute details are given as to the preparation of the surgeon and patient previous to operation. For the text we have nothing but praise, though doubtless most surgeons will find that some of the methods advocated differ somewhat from their own practice. The illustrations, on the whole, might be much improved; most of the reproduced photographs are very blurred, and even the coloured plates do not reach a high standard. We can highly recommend the book as being very readable and unusually practical. It carves out a line for itself, and instead of giving all sorts of methods of accomplishing the same end—a fault of many textbooks—it gives the views of one who has had great experience. It is, therefore, dogmatic about many things which are at present open to question, but this is an advantage to any one who really requires to be told what to do and how to do it.

The second edition of *Diseases of the Eye*,[3] by Mr. J. HERBERT PARSONS, much resembles its predecessor in appearance, but it has been revised and some new matter added. It is divided into six sections and three appendices. The illustrations, which are numerous, are both coloured and black and white; a great many of these are borrowed, but some are original. The pathological aspect of the various subjects dealt with is treated with rather more detail than is usual in books of similar size, and the same may be said of the optics. The author clings to the old theories and tests for colour blindness, which the majority of workers have found to be unsatisfactory and at variance with the facts arrived at from examining persons who are affected in this way. The nervous mechanism connected with the eye is fully treated and the whole book is well arranged and written.

[2] *Textbook of Ophthalmology in the Form of Clinical Lectures.* By Dr. Paul Roemer, Professor of Ophthalmology at Greifswald. Translated by Dr. M. L. Foster, of New York. Vol. I. London: Rebman, Ltd. 1912. (Sup. roy. 8vo, pp. 291; figs. 186, plates 13. 10s. 6d. net.)

[3] *Diseases of the Eye.* A Manual for Students and Practitioners By J. Herbert Parsons, D.Sc., M.B., B.S., F.R.C.S. Second edition. London: J. and A. Churchill. 1912. (Demy 8vo, pp. 684; 309 figures, 12 plates. 12s. 6d. net.)

Among the books in the useful series of Oxford Medical Publications is one on *Diseases of the Eye*,[4] written by Mr. STEPHEN MAYOU. It has now reached a second edition (the first was published in 1908), and the new issue presents this exceptional feature—that it contains only 306 pages, or 82 less than the first. The tendency of most textbooks is to grow more bulky. For so small a book it is profusely illustrated, containing 124 pictures in the text and five plates, four of which are reproductions in colours of ophthalmoscopic drawings. The new edition is a decided improvement on its predecessor, and there are fewer accidental errors in it. But one curious mistake has crept in. When describing Snellen's test types the first edition told us that the letters subtend an angle of 5° on the retina at the distance marked over each; this is, of course, absurd. In the second edition we are told that the angle the letters subtend is 5″, which is certainly going from one extreme to the other; the correct angle is 5′. In so small a book the description of the various conditions spoken of must be rather sketchy; but the work is well done, and we are glad to see that the necessity has arisen for the issue of a second edition. Such a book is bound to be popular with students.

INFANT FEEDING.

DR. BOSC, of Montpellier, published some months ago a careful essay[5] on breast feeding, artificial feeding, and mixed feeding, and on the diet of children from 1 to 3 years old. He has special chapters on the dieting of feeble infants and those with hereditary syphilis. He states that contamination of a healthy nurse by a syphilitic infant is frequent, and laments the fact that only too often the nurse subsequently infects her own children and husband. If the author speaks from a personal knowledge of the frequency of such lamentable cases, we can only say that his experience must be wholly exceptional. In a final chapter he deals with the excellent results achieved in the lowering of infantile mortality by the institution of "infantile consultations" at different places in France, and gives some details as to the working of his own at Montpellier. He speaks in glowing terms of the marvellous (*réellement merveilleuse*) effects of mountain air, at elevations of from 3,000 to 3,500 feet, in the prevention and treatment of summer diarrhoea in infants, and states that removals to such altitudes have reduced the mortality from 80 to 3.8 per cent. We can readily believe in the efficacy of such air as a preventive, but have a difficulty in understanding how infants could be taken to it in the acute stages of the disorder, fortunate as Montpellier may be in having mountains of such elevation close at hand and readily accessible.

Dr. D. HASTINGS YOUNG, in his book, *Medical Education and Infant Feeding: An Impeachment and a Suggested Remedy*,[6] carries us over a wide field. From a consideration of the difficulties of digestion in infants, his argument leads us by gradual steps to an "impeachment" of the General Medical Council. The author is an ardent believer in the value in infantile dietetics of humanized milk prepared according to a formula devised by Dr. Truby King, of New Zealand. He has himself used it on five occasions, and he gives the results in the form of a table. In three cases the children did well, one did fairly well, and one developed diarrhoea upon the sixth day, and the food was changed. The author brings forward these results as evidence in support of his contention that "the acme of our present knowledge of infant dietetics is reached by the use of Dr. Truby King's formula," and maintains that official recognition of the method is necessary in this country. He complains of the want of uniformity in the practice of medical men in infantile dietetics and of their lack of interest in the subject, and this he attributes to a want of special courses of instruction in medical schools. This lack of instruction is in turn the result of the apathy of the General Medical Council, and the apathy of the General Medical Council is due to faults in the composition of that body. "As specialists they have legislated in the interests of specialists and have formed themselves into a kind of House of Peers of self-sufficient, superior persons." Finally, the argument culminates in the impeachment, "It would almost appear as if the policy of the British (*sic*) Medical Council were to encourage entry into the students' ranks and to hinder the successful completion of the course with a view to personal gain." The author in this argument has, as will be seen, travelled a long way from his starting-point. With the author's plea that the study of infantile disorders should find a place in the curriculum, whether as a special subject taught apart from general medicine, as the author suggests, or, as at present, as a part of ordinary medical teaching, it is impossible to disagree. It is not easy, however, to follow the author in his further arguments that the difficulties of the subject would be diminished by the uniform adoption of any one form of modification or humanization of milk, and by stamping it with the seal of official approval, nor is it easy to agree that the present crowded state of the students' curriculum is due to the dishonesty or incapacity of the General Medical Council.

DISEASES OF THE MOUTH.

IN *Stomatology in General Practice*[7] Dr. H. P. PICKERILL tries to tell the general practitioner of medicine what he should know of oral conditions. The book is comprehensive, and reference is made to almost every condition of disease which occurs in the mouth, but it is of unequal merit, and in places spoilt by neglect of facts which run counter to the author's theories. He believes that loss of function in mastication is mainly responsible for deformities of the jaws, and neglects those idiot children who, though fed from birth till death on "slops," yet have jaws above the average in size. On p. 158 arrest of development of an ankylosed mandible is ascribed to loss of function—but may it not be due to destruction of the growing centre? The author's conception of dental sepsis appears to be somewhat limited; stroking the gums to express pus (p. 218) is the extent of his examination for periodontal infection, and his only reference to the septic origin of hypertrophy of gums and alveolar bone is that "it has been suggested that the origin is always an infection" (p. 183). There is much else to criticize adversely, sometimes in grammar (p. 14, par. 2), sometimes in fact; how many times has the author found a tooth entirely surrounded by an epithelial lined cavity (p. 172)? Taken as a whole, however, the book is well worth reading. Throughout it there are original ideas and observations which must set the reader thinking, and some of the chapters—for example, treatment of dental disease, neuralgias—are excellent. Most of the micro-photographs are poor, spoiling an otherwise good set of illustrations.

PUBLIC HEALTH LAW.

THERE is already, both for the student and for those who are actively engaged in administrative duties, such a large choice of textbooks relating to public health law that there would hardly seem to be room for any addition. Mr. BERTRAM JACOBS has, however, managed to incorporate in his *Manual of Public Health Law*[8] some matter which is either not to be found at all in other textbooks or which has been dealt with only very indifferently. For example, the chapter on the public health authorities gives a clean and well-marshalled account of the public authorities which are concerned with the administration of the laws relating to public health, and he distinguishes in a fairly correct manner between the powers and duties of urban and rural authorities, though he might perhaps have entered into more detail when discussing the latter. For instance, he has omitted to point out that slaughterhouses in rural districts do not come under any of the provisions of the Public Health Acts unless the urban powers relating

[4] *Diseases of the Eye.* By M. Stephen Mayou, F.R.C.S. Second edition. Oxford Medical Publications. London: Henry Frowde, and Hodder and Stoughton. 1912. (Cr. 8vo, pp. 320; figs. 124; plates 5. 5s. net.)

[5] *Précis d'alimentation du nourrisson allaitement et sevrage.* By Frédéric J. Bosc, Professeur à la Faculté de Médecine de Montpellier. Paris: Masson et Cie. 1911. (Cr. 8vo, pp. 258.)

[6] *Medical Education and Infant Feeding: an Impeachment and a Suggested Remedy.* By D. Hastings Young, M.B., Ch.M.Edin. London: Routledge and Sons, Ltd. 1911. (Post 8vo, 120 pages. Price, 2s. 6d.)

[7] *Stomatology in General Practice: a Textbook of Diseases of the Teeth and Mouth for Students and Practitioners.* By H. P. Pickerill, M.D., Ch.B., M.D.S.Birm., L.D.S.Eng., Professor of Dentistry at the Otago University, N.Z. London: Hodder and Stoughton. 1912. (Demy 8vo, pp. 280, illustrations 65. 15s. net.)

[8] *A Manual of Public Health Law.* By Bertram Jacobs, Barrister-at-Law. London: Sweet and Maxwell, Limited. (Demy 8vo, pp. 220. 7s. 6d. net.)

to them have been allowed by the Local Government Board, nor does he state that in a rural district a newly erected house cannot be occupied unless certified to be provided with a sufficient water supply. We do not find, moreover, any mention of the law relating to knackers' yards. These minor omissions do not, however, materially militate against the usefulness of the work, an important feature of which is the large number of references to cases which have been decided in the courts; those quoted, moreover, have been brought quite up to date. Should a second edition be brought out, we would suggest to the author that he should compile a much more extended index to the contents.

The study of the various laws relating to public health is not made any easier for the student by the fact that England and Wales, Ireland, Scotland, and London each have separate Acts of Parliament dealing with public health administration. With a view to lessening those difficulties Dr. ROBERTSON and Dr. MCKENDRICK have produced a work on *Public Health Law*[9] which they describe as an epitome of law applicable to England and Wales and Scotland. They deal with the subject in three parts under the headings of Acts specially applicable to Scotland; Acts applicable to England and Wales; and Acts applicable to the United Kingdom, though it would be more correct to have stated that the third part dealt with Acts applicable to Great Britain. The sections of the various Acts are set out in conspicuous type, and useful notes are appended in many instances. There is, however, evidence of some carelessness in compilation. This is particularly noticeable in the section dealing with the Housing of the Working Classes Acts, for the authors have apparently overlooked the fact that important sections and parts of sections of the Act of 1890 were repealed by the Housing, Town Planning, etc., Act, 1909. The work is, however, generally well up to date, for it contains the Rag Flock Act, 1911, and the regulations made under that Act which came into force as recently as July last.

NOTES ON BOOKS.

To Dr. J. T. RICHARDS, Chairman of the Health Committee of the Battersea Borough Council, belongs the distinction of having written the shortest account of the Insurance Act giving anything like an adequate analysis of that complex measure. Into an extraordinarily small space he has packed a consecutive account of its provisions which will be intelligible to the general public to whom it is addressed. It could only have been done by one who had studied the Act very thoroughly. There are points of interpretation as to which every one will not agree, and we shall probably not know exactly what the Act means in some parts until the law courts have had their innings. Meanwhile, Dr. Richards can give clause and subclause or very good reasons for everything he says, and his little pamphlet,[10] which he describes as a plain exposition of the Act for employers and employed, will be very useful.

It is no easy matter to form a just and correct estimate of Mr. C. E. STROMEYER'S book, *Unity in Nature*.[11] Almost every branch of human knowledge is dealt with; music and morals, physical and natural sciences, ethics and philosophy, eugenics and politics. Neither is it very clear what is the central idea the author has in his mind and wishes to convey to his readers. The title would seem to furnish the clue, but the book itself fails to afford much elucidation. In the introduction we are told that the first task which the author has undertaken is "to convey the idea that music is an independent world" and that the writing of the book has been "prompted by Schopenhauer's suggestion of a parallelism of music and life." In evolving this idea

the conviction grew that indestructible energy, a comparatively recent discovery, in all its numerous manifestations as Life, deserved far more attention than has yet been accorded to it by metaphysicians, and that possibly a better use than has been customary might now be made of the great advantage possessed by human beings of being able to contemplate all things, and that this might lead to a clearer understanding of what perfect life is or what it should be.

The whole book is highly speculative, and it would not be difficult to join issue with the author in many of his ideas. Some of the chapters dealing with social problems are of interest and contain much common sense, but we think that the author will not find many who will agree with his ideas on sexual ethics. Whatever future generations may think, the world is not at present willing to accept the views here put forward. The book undoubtedly contains much to interest, and shows wide knowledge, but we confess to leaving it with feelings akin to those experienced after having partaken of a heavy meal of indigestible food, feelings of repletion and yet of unsatisfaction.

The importance of securing the systematic inspection of all foodstuffs is now so generally recognized that it has been necessary for sanitary officials to specialize in the subject, and in most sanitary districts of any considerable size one or more inspectors are employed whose sole duty is the inspection of foods. In small localities the inspector of nuisances is expected to include such inspections among his ordinary duties, and it is to be feared that as a rule he has no very special knowledge of the subject. To such Mr. HORACE WALKER'S *Food Inspectors' Encyclopaedia*[12] should prove of great assistance. It can hardly take the place of the larger textbooks, but the alphabetical arrangement lends itself to ready reference, and the information given is accurate and precise, while the practical advice which abounds in the volume is evidently the result of first-hand knowledge obtained from actual experience. Of particular importance is the long list of the most common ways of adulterating food, with an account of some substitutes and other tricks by which purchasers are frequently defrauded. As a handy book of reference the work should be of value to all public health officers.

The third edition of Savill's *System of Clinical Medicine*[13] is due to the energies of Dr. AGNES SAVILL, and may be regarded as thoroughly up to date, since apart from ordinary revision it includes a considerable amount of new matter in the way of notes on many conditions comparatively rarely seen in this country; the notes on Rocky Mountain fever and rat-bite fever are cases in point. It is less than ten years since the first edition appeared, and the present edition is faithful to the original idea—namely, that of putting observed effects in the foreground and endeavouring to trace them back to their causes. This is a sufficiently common plan in bedside teaching, but the late Dr. Thomas Savill, who was in many respects an original-minded man, was the first, we believe, to endeavour to apply it to a volume of the treatise order. Many of his dicta, perhaps, are open to discussion, and the book as a whole is not, perhaps, every one's book; it nevertheless contains an immense amount of information, and the plan on which it is arranged must clearly appeal to a considerable number of workers in medicine, since both the first and second editions were rapidly exhausted. Corresponding success for the present edition may be anticipated, since the level which it attains is fully equal to that of its predecessors, while the ground which it covers is even wider.

The fact that Miss MARGARET D. PALMER'S book, *Lessons on Massage*,[14] has reached a fourth edition is evidence that it is found useful by a number of nurses and students in massage schools. Of its sixteen chapters eight are devoted to elementary anatomy. The rest are given over to massage in its various forms, the Weir-Mitchell system and its diet, Nauheim treatment, and bandaging. The anatomical descriptions are somewhat loose. For instance, what is a nurse to make of the following: "The pelvis is divided into true and false. The false pelvis *is the* (*sic*) abdomen proper, the part that is enclosed in front only by muscle and fascia. The true pelvis, or pelvic cavity, is the part which is bounded in front by the pubes, the upper surface of which is the

[9] *Public Health Law; an Epitome of Law Applicable to England and Wales and Scotland.* By William Robertson, M.D., D.P.H., and Archibald McKendrick, F.R.C.S.Edin., D.P.H. Edinburgh: E. and S. Livingstone. 1912. (Cr. 8vo, pp. 408. 5s. net.)

[10] *National Health Insurance; a Plain Exposition of the Act for Employers and Employed.* By J. T. Richards, B.Sc., M.B. Third edition. London: Wm. Pull and Co. and W. H. Smith and Son. (Fcap 8vo, pp. 16. 2d.)

[11] *Unity in Nature: An Analogy between Music and Life.* By C. E. Stromeyer, Past Member of the General Board of the National Physical Laboratory, etc. London: Sherratt and Hughes. 1911. (Med. 8vo, pp. 599. 12s. 6d. net.)

[12] *The Food Inspectors' Encyclopaedia.* By A. Horace Walker. London: Baillière, Tindall, and Cox. 1912. (Demy 8vo, pp. 311. 7s. 6d. net.)

[13] *A System of Clinical Medicine.* By Thomas Dixon Savill, M.D.Lond. Third edition, thoroughly revised. London: Edward Arnold. 1912. (Med. 8vo, pp. 958. 25s. net.)

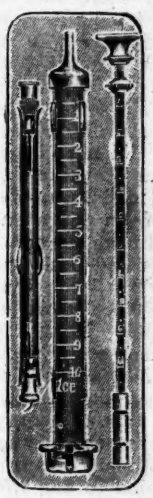
[14] *Lessons on Massage.* By Margaret D. Palmer, formerly Masseuse and Manager of the Massage Department of the London Hospital, etc. Fourth edition. London: Baillière, Tindall, and Cox. 1912. (Med. 8vo, pp. 308; figs. 118. 7s. 6d. net.)

trim. The ridge [what ridge and where?] is called the ilio-pectineal line. The abdomen proper is in a nearly straight line from the thorax," etc. The book is well printed, and the plates are clear, and the massage teaching is simple and sensible; it will no doubt continue to be useful to many students of massage.

MEDICAL AND SURGICAL APPLIANCES.

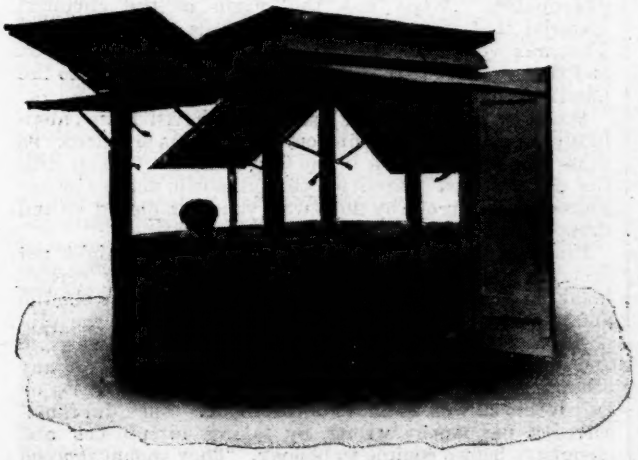
A Syringe for Tuberculin.

DR. H. EWAN WALLER (Birmingham) writes: Now that tuberculin is being so much more widely used in the treatment of consumption in this country, it becomes of increasing importance for the practitioner to be able to prepare his own dilutions. One of the chief difficulties has been the need of a suitable syringe for the purpose. The calibre of an ordinary hypodermic syringe is much too great. In order to measure $\frac{1}{10}$ of a cubic centimetre (0.1 c.cm.), one had to move the piston of the syringe approximately $\frac{3}{33}$ in. Obviously, an error of even $\frac{1}{64}$ of an inch in movement introduces an error of 1 in 6 in the resulting dilution. This error may be repeated several times before one has arrived at an ordinary initial dose of tuberculin, such as 0.0002 c.cm. P.T.O. I have had made for me by Messrs. Anderson and Dodds, of Broad Street, Birmingham, a syringe of much finer calibre, in which the piston has to be moved about $\frac{5}{16}$ of an inch to measure 0.1 c.cm. The possible error is, therefore, 1 in 20 instead of 1 in 6 for each sixty-fourth part of an inch traversed by the piston. This is obviously a great gain. The syringe itself is much the same in type as the "Metalline" and "Record" syringes. It is all metal and glass, without washers, and is put up in a metal case with the plunger separated from the barrel, which obviates the risk of finding the two inseparable at any time when the syringe has not been recently used. The piston rod of the syringe is also graduated and provided with a screw attachment, which is an additional safeguard against error in measuring. The capacity of the syringe is 1 c.cm.



Open-Air Shelter.

Dr. Hollick (Knowle, Warwickshire) sends us the accompanying photograph of an open-air shelter for early cases of tuberculosis made by Mr George Sumner, carpenter, of Knowle. Dr. Hollick has three of them in use and finds them excellent. They can, he states, easily be taken down and removed through an ordinary doorway or entry.



The following is the carpenter's specification: "These shelters are made of best red deal. Framework 3 by 2, floor joist 3 by 3, floor 1 inch tongued and grooved boards; ends, front, and back boarded with $\frac{3}{4}$ matchboards, roof matchboarded and covered with 1 ply rubberoid, two coats of creosote outside, two coats of white paint inside. All shutters open with strong stays. Made in seven sections and can be taken through any ordinary doorway or entry, price £10."

ROYAL MEDICAL BENEVOLENT FUND.

AT the August meeting a number of cases were considered, and grants amounting to £191 made to nineteen of the applicants. Appended are particulars of the cases relieved:

Widow, aged 45, of L.R.C.P., L.R.C.S.Edinburgh. Practically unprovided for at husband's recent and sudden death, and hopes to support herself by taking boarders. Three children, aged 17 to 14½. Voted £10 in one sum.

Widow, aged 57, of M.B.London, who was for many years an annuitant of this Fund and of Epsom College. No income; only son unable to help at present. Proposes to maintain herself by receiving paying guests. Voted £10 in one sum.

Widow, aged 33, of M.R.C.S., L.S.A., who practised in Cheshire. Since husband's death about four years ago has supported herself by nursing. Has recently been promised a post as matron at a good school, where her daughter, aged 9, can receive a thorough education for a nominal sum, but requires help for suitable clothing and maintenance meanwhile. Voted £10 in one sum.

Widow, aged 69, of late M.R.C.S., who practised in Berkshire. Has a small pension but is in bad health, and has recently had heavy and unavoidable expenses. Voted £5 in one sum.

Widow, aged 57, of M.R.C.S., who practised in London. Is in very feeble health, and dependent entirely on a step-brother whose help is both small and uncertain. Voted £10 in two instalments.

M.B., B.C., aged 53. Has broken down through overwork and requires a long rest, for which his means are quite inadequate. Voted £20.

Daughter, aged 73, of late M.R.C.S., who was a surgeon in the Scots Guards. Was well provided for but lost everything through a defaulting trustee, and is now dependent on a small annuity purchased by friends and an old age pension of 3s. a week. Voted £10 in two instalments.

L.R.C.P.Edinburgh, aged 68, who practised in London. Used to have a good practice, but is now incapacitated by the effects of an accident, and is dependent, with an invalid daughter, on his wife, whose income, including temporary help from a brother, is only about £1 a week. Relieved three times, £54. Voted £18 in twelve instalments.

Daughter, aged 66, of late M.R.C.P.London, F.R.C.S., who practised in Derbyphire. Health very poor, and only income a small pension from a charitable society. Relieved five times, £37. Voted £5 in one sum.

Daughter, aged 63, of late M.R.C.S., who practised in Warwickshire. Maintained herself for many years by dispensing, but is now unable to obtain regular work and is practically dependent on a sister who has to earn her living. Relieved six times, £60. Voted £10 in ten instalments.

Widow, aged 55, of L.R.C.P.Edinburgh, who practised in Staffordshire. Since husband's sudden death seven years ago has maintained herself by needlework and occasionally writing for the papers. Two daughters, the elder just self-supporting, the younger still at school. Relieved seven times, £72. Voted £10 in ten instalments.

Widow, aged 41, of M.B., Ch.B.Edinburgh, who practised in Surrey. Has an income of £15 a year, but is unable to undertake regular employment on account of her mother's ill health. No children. Relieved once, £5. Voted £5 in one sum.

Daughter, aged 56, of late M.R.C.S., who practised in Suffolk. No income, and practically incapacitated by bronchitis and chronic rheumatism. Relieved ten times, £97. Voted £12 in twelve instalments.

Widow, aged 42, of M.B., C.M.Aberdeen, who practised in Wales. Receives a small salary as housekeeper, but finds it insufficient for the maintenance of her two sons during the holidays, one of whom is at the Bluecoat School. Relieved five times, £45. Voted £12 in twelve instalments.

Widow, aged 41, of M.R.C.S., L.R.C.P., who practised in India. Since husband's death from plague fourteen years ago has been very generously helped by his brother, whose allowance is unavoidably withdrawn for anyhow a few months. Three children, the eldest 19, the two younger both delicate. Relieved three times, £20. Voted £10 in one sum.

Widow, aged 78, of L.F.P.S.Glasgow, who practised in Hampshire. Receives rooms and 10s. a week at an almshouse, but is paralysed, and consequently obliged to pay for an attendant. Children unable to help. Relieved twice, £17. Voted £5 in one sum.

Widow, aged 51, of M.B., C.M.Edinburgh, who practised in Perthshire. Supplements an income of £20 a year by taking boarders, and has brought up a family of seven children, of whom two are now self-supporting. Relieved thirteen times, £150. Voted £12 in twelve instalments.

Daughter, aged 45, of late M.R.C.S., who practised in India. Is a trained nurse, but is permanently incapacitated as the result of pyaemia. Only income a pension of £30 a year from a charitable society. Relieved once, £12. Voted £12 in twelve instalments.

Daughter, aged 61, of late M.R.C.S., who practised in Gloucestershire. Lent her capital to a friend whose security proved valueless, and is now dependent on a pension of £21 a year recently granted by the Royal United Kingdom Beneficent Association. Relieved eight times, £104. Voted £5 in one sum.

Contributions may be sent to the Honorary Treasurer, Dr. Samuel West, 15, Wimpole Street, W.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

(Concluded from page 647.)

COMBINED MEETING OF SECTIONS OF ZOOLOGY AND BOTANY.

The Origin of Life.

DR. E. A. MINCHIN, in introducing a discussion on this subject, said the problem of the origin of life involved two inquiries, both at present of a speculative order: the first, as to the nature and characteristics of the earliest living beings; the second, as to the manner in which the primordial form of life took origin and maintained its existence upon the earth. I. The *cell*, which might be defined as an individualized mass of *protoplasm* containing at least one *nucleus*, had generally been regarded as the simplest type of organism, the vital unit in the composition of living beings, whether plants or animals. It was, however, improbable that the earliest forms of life came into existence as organisms composed of two distinct and separate structural elements, the *nucleus* and the body protoplasm or *cytoplasm*. Which, then, was to be regarded as representing or containing the most primitive elements of the living substance, the cytoplasm or the nucleus? By most biologists the cytoplasm had been considered to represent the true living substance. The earliest living beings had been supposed to be formless masses of protoplasm without nuclei, so-called Monera. There were, however, many reasons for believing that the chromatin substance, invariably present in the nucleus, or occurring as grains, chromidia, scattered in the cytoplasm, represented the primary and essential living matter. In support of this view might be urged:

1. The fact that chromatin is always present in the bodies of living organisms of all kinds.
2. The experimental proof that cells cannot continue to live if deprived of their nuclei.
3. The fact that in reproduction by fission the chromatin divides first, and is distributed among the daughter indviduals; karyokinesis may be regarded as a mechanism gradually evolved and perfected to ensure an exact quantitative and qualitative partition of the chromatin between the daughter nuclei.
4. The relation of the chromatin substance to syngamy and probably to heredity.
5. The part played by the nucleus in the production of ferments in the cell.
6. The fact that in the minutest living organisms the body appears to consist mainly or entirely of chromatin, cytoplasmic elements being either reduced to a minimum or absent altogether.

For these reasons Dr. Minchin regarded the chromatin as the primitive living substance, and held that the earliest forms of life were very minute particles of chromatin, round which in the course of evolution achromatinic substances were formed. Within the cytoplasmic envelopes thus produced the chromatin grains increased in number. Organisms of the degree of structural complexity of a true cell arose finally by concentration of the chromatin grains (chromidiae) into a compact organized mass, the *nucleus* proper. II. As regards the origin of the earliest living beings, it was only possible to frame vague speculations in the present state of knowledge concerning the chemistry of the protein compounds on the one hand and the metabolism and modes of life of the simplest living things on the other. Whether life originated on the earth itself, as biologists had generally supposed, or was brought in some way to the earth from infinite space, as some physicists had suggested, its first origin involved a synthesis of protein substances in Nature by some process as yet totally unknown. When the earliest form of living being had come into existence, it was very difficult to understand how it could have maintained its life and what it could have fed upon, especially if it were originally wafted from space on to a sterile and barren earth. For light on these problems they must look to the future advance of knowledge, and especially of chemical science.

Professor H. WAGER said they might have gathered from Professor Minchin's interesting statement that they were, as they would say in Yorkshire, "Not very much forrarder" as regards a solution of this extremely important problem. They must regard the fundamental life substance as cytoplasm, and not chromatin, and he thought they would agree that it was not altogether satisfactory that Professor Minchin should adduce as his main argument in favour of chromatin that it was the structure of an organism, the nature of which he definitely and distinctly said was not yet known.

Professor KEEBLE (Reading) said both chemical and mechanical theories had been abandoned in favour of a theory which regarded life as a factory containing all different kinds of machinery, and where they put in a pig at one end and obtained mincemeat at the other, or put in mincemeat at one end and got a pig at the other. It seemed to those who were perplexed with the complexity of life that, so far, the views expressed on it had not convinced them that they would see what was called synthetic life in the immediate future.

Professor MACALLUM (Toronto) thought matter was endowed with the potentiality of life, and to that extent he was in sympathy with the view that there was an association between them. He had enunciated the view that the organism which first came into existence was an ultra-microscopic one. At one time the earth was a vast laboratory, and in its atmosphere were an enormous pressure and electrical current and all the compounds which were simply necessary for the process. Synthesis must have occurred at that time which did not obtain to-day. Everybody who talked about the origin of life spoke of it as a great big cell. That was a mistake, as the cell they found to-day was as far removed from the ultra-microscopic organism as they were removed from the cell.

Professor MOORE (Liverpool) said the view had been put forward that if they were able to get over all the difficulties of the chemist and fabricate a substance identical with protoplasm they then would have life. That was purely a static view of life, and depended entirely on structure. It seemed useless for the biologists and physiologists to shut their eyes to the fact that they had a separate science, that they had a separate type of energy which was in a distinctive state of phenomenon, just as radio-activity was different from the ordinary electric waves. There was a peculiar transformation in the life cell. The first thing they had to evolve was an organic molecule from the simpler inorganic ones. It was not a metaphysical but an experimental problem, and would yield fruit as soon as it was attacked. The President had told them there might have been various times when life was produced on the planets. He believed at present, and all through the ages just as at various levels, that process was going on. Chemistry and physics must be the underlying elements in the whole process. They must remember that there was a plan running through the whole of evolution. They must call in the psychologist.

Professor MACDONALD said it seemed to him they had a limited problem such as even at the present time Professor Minchin was perfectly justified, and science was perfectly justified, in presenting to the mind of the world for its examination. What was the origin of the chemical material that was found in the nucleus of living cells? That was what Professor Minchin's paper boiled down to, and that was a matter which had to be left entirely to the hands of the chemists.

Professor HARTOG (Cork) said he believed it was absolutely hopeless to try and find out how life was produced. To those who had ambition in the field he said: "Go on with the disinterested search for the unknown, and we or our successors will profit by the jewels you have picked up and dropped on the way."

Professor PATRICK GEDDES asked if they might not spend a moment in looking how, without any serious break in the scientific order, they might look at the life and mind together. He entirely dissented from the proposition of the President that they could really suspend and annihilate the psychic life by anaesthetics or any other process. The life process went on at one action on its psychic as well as on its organic side. They did themselves a permanent wrong by taking in physics and chemistry before coming to biology. They should proceed from life outwards towards the environment. To discover the origin of life was to trace the action of one's own life, in which one never lost sight of the unity of reaction, organism, and environment.

Dr. J. S. HALDANE (Oxford) looked for a future life to the more exact but more careful study of the things of what were called molecules and atoms. In studying the primitive forms of life they were extending the organic world into the apparently inorganic.

Dr. STEBBING (Tunbridge Wells) said the evolution of life might be going on continuously. They had not seen it, and had not been able to reproduce it by experiment, but that was no reason for thinking it could not happen.

Dr. CHALMERS MITCHELL (London) said they were getting closer and closer to the last difficulty in the whole business—as to whether there was a distinction between the properties and qualities of living matter and of matter which was not alive. There had not been a single property of protoplasm produced to-day which had not its exact physical equivalent.

SECTION OF PHYSIOLOGY.

Prevention of Mental Degeneracy.

A paper by Dr. R. R. RENTOUL on the prevention of mental degeneracy suggested the following steps: (1) Compulsory notification of all mental and physical degenerates. (2) Compulsory and voluntary surgical sterilization of mental and physical degenerates. (3) Prenuptial medical certificate of good physical and mental health. (4) Making it illegal for any person to issue a permit to marry without obtaining beforehand a prenuptial certificate of good health from the applicants. (5) Making it illegal for any clergyman, registrar, or other person to join in marriage any physical or mental degenerate unless the woman is over 50 years of age, or unless sterilized. (6) Making it illegal for any sane person to marry a degenerate, and to make such marriage null and void. (7) Taxation of bachelors. (8) Taxation of sterile marriages. (9) Proceeds to go to couples with income under £450 per annum who bring up the largest number of physically and mentally healthy children. (10) Petitioning Her Majesty to institute a Royal Order of Maternity. (11) Physical degeneration.

A Plea for Regeneration.

Viewing the topic from another standpoint, the Rev. JAMES MARCHANT, Director of the National Council for the Promotion of Race Regeneration, said that degeneration, whether attacking the individual or the race, in a physiological or mystical sense, was not peculiar to this age. And the fact of regeneration was well known. But if the belief in regeneration was not novel, the application of it to the race at a time when belief in degeneration had seized upon the public imagination was a primary duty of physiologists and biologists, and of every one who had not lost faith in our ultimate recovery from the prevailing nervous attack. Quantitative human life, as shown by the falling birth-rate in some Western countries, whether the immediate causes were voluntary or not, might seem to be failing, but might it not be only a seeming? What new physiological fact of the reduction of essential fertility had come to light since 1875, when the birth-rate began to fall? Or had any inherent tendency towards a progressive decline in the standard of physique been disclosed? Was it not still true that nearly 90 per cent. were born healthy? And that many of the defects in adults and in school children were due to ignorance and avoidable social conditions? Was it not plain that qualitative life (which after all was in fact the only true way of measuring life) was advancing? That was a sociological fact, but was not its foundation, whether seen or unseen, physical? Might not the full explanation of evolution demand constant regeneration of life? The evolution of society was founded upon it. Physiology was concerned with the processes and functions of vital—that is, regenerating—organisms. Religion preached regeneration unceasingly; apart from it her faith and mission would be mockery. Dr. Marchant pleaded that the time had come to turn public attention and popular faith to the more assured belief in regeneration, and to throw the potent influences of science, sociology, and religion into this scale. "Put men into good physical surroundings, with opportunity to encourage healthy mental, moral, and spiritual development, make them less introspective and self-centred, and we shall soon have regeneration."

SECTION OF ZOOLOGY.

Speech in Animals.

In a communication on this subject Professor R. J. ANDERSON said there was an obvious relationship between the expression of wishes, emotions, etc., by means of words and the capacity to understand words. Many seemed to believe that a dog, cat, or horse could be trained to understand the common meaning of many words. On the other hand, many believed that even the monkey, elephant, or parrot had a very limited capacity in this regard, and that the tone of voice, pose or gesture counted for much with many animals. Association and the reflex accompaniments seemed to count for much. A little dog would learn very soon to associate the presence of some friend with the licking of his lips. Salivation, which was commonly associated with the smell, sight, or thought of food in man, became in a dog associated with a nerve stimulus or sound, as the experiments of Pawlow and his pupils proved. The bell that calls the workman from labour to refreshment might lead a dog to "whine" or bark because of its association. A dog distinguished between a lively tone and menacing tone; this might be the result of education. And the sense of smell was evidently the most powerful associated sense. Sight and hearing were not so keen as scent in dogs. The power and disposition to seize upon some salient feature by the eye or ear was easily recognizable. This "quickness" was apt to lead to fatal errors, but answered the main purpose. The sounds that animals emitted were apparently, except in man, limited to a few often guttural or modified guttural expressions. The bark of a dog varied in loudness and in quality. The main cause of the sound was alarm, or a desire to alarm or attract the attention of another dog or the master. It was evidently the most useful sound a dog possessed. A dog was probably first domesticated because of its bark. The whine was a sound of appeal. Thus Franke's dog whined in order to get a box lifted; there was a bone beneath the box. Entreating sounds, cries for help, also threatening and warning cries were common amongst many animal tribes. Pain, fear, and anger might cause a dog to howl. Some animals did not express a sense of pain unless it were intense. Certain sounds made a dog howl. This might arise primarily from discomfort, but might afterwards arise from association. A cat whimpered and mewed mildly when she wanted a door opened. Her mew had a different tone when she was angry or hungry. Little guinea-fowl and pheasants soon learnt to run over to a "sod" to wait in expectation of a visitor turning the earth up to expose the worms. The bark, the tone of alarm was akin to the noise made by monkeys. It might, perhaps, develop into a battle cry, as a number of barking dogs would tend to intimidate adversaries, as happened in the case of men. The early training of dogs, horses, and other animals went far to obliterate any tendency to marked development of original lines of thought. It was to be doubted whether any great advance could be made in the development of a dog language. But that a series of combinations might be possibly made out of the sounds which adult dogs had at their disposal could not be doubted. The study of young dogs would be more fruitful in view of the fact that the vocal apparatus might be modified.

Parasitic Worms.

Dr. W. NICOLL gave an account of recent progress in our knowledge of parasitic worms. Dealing with the subject in all its various aspects, the address was of interest not only to zoologists, but also to medical men and veterinarians. Most attention was naturally devoted to matters of purely zoological importance, but economic and medical questions received as full treatment as time would permit. Dr. Nicoll dealt with the work of the last four or five years, and laid particular stress on the value of the advances in classification, morphology, and development made during that period. From the large mass of investigations which have been accomplished in this short period, the speaker selected the chief results of the most outstanding and best known authorities. Foremost amongst those he placed Looss, Fuhrmann, and Odhner, who have been mainly responsible for the great advances made in our knowledge of the flat-worms. Others whose work was specially mentioned were Railliet, von Linstow, Stiles, Ward, Lühe, Weinberg, and numerous other well-known helminthologists. Such a long list of distinguished names is evidence of the serious and extensive attention which the subject has been receiving. Of systematic work, the bulk has been devoted to Trematodes, and many of the subdivisions of this class have been very thoroughly treated. The *Echinostomes*, for instance, the *Hemiuridae*, the *Paramphistomes*, and the *Monostomes*, have all been very amply dealt with. The great wealth and variety of

bird tapeworms, again, has been made a special study by Fuhrmann and by Ransom, while the tapeworms of mammals have also received some attention. The outstanding systematic work on nematodes has been that of Railliet and Henry, who in a series of short contributions have elaborated methods of classification which appear to be of permanent value. With regard to faunistic work Dr. Nicoll remarked on the constant addition of new and interesting forms, but at the same time he expressed the opinion that an enormous wealth of parasitic material remained entirely untouched and unknown. Outside Europe, only in the United States and in the valley of the Nile had any extensive attempt been made to investigate the parasitic fauna. A good beginning has been made in Australia, but Asia and South America are practically *terrae incognitae*. It is impossible to mention here the numerous morphological matters discussed in the address. Chief of these is the discovery made by Goldschmidt in 1909 that in the flat-worms the eggshell is not secreted by the so-called shell-gland, but by the yolk-glands, which have thus a double function, and possibly their nutritive function is the less important. Another interesting matter is the demonstration, based on a study of the nervous system, that what is usually regarded as the head of a tapeworm is in reality its posterior end. The most outstanding contribution to nematode anatomy is that of Looss on the human hookworm. This work is already familiar to most of our readers. Passing on to consider the bionomics and life-history of some of these forms Dr. Nicoll mentioned the discovery of the intermediate stage of the Japanese liver fluke (*Clonorchis endemicus*) and of the mode of infection of the Japanese blood fluke (*Schistosomum japonicum*). In the same connexion he also again referred to Looss's work on the hookworm (*Agchylostoma*), and to that of Leiper on the guinea-worm (*Dracunculus*). In regard to verminous toxins and their action in producing disease, reference was made to the recent work of Elmy and of Weinberg, a discussion on which took place at the annual meeting of the British Medical Association this year. The helminthic affections which have attracted most attention during the past few years have been schistosomiasis, ankylostomiasis, and hydatid disease. Considerable attention has been paid to the pathology of the first mentioned, but the most important new fact in regard to it is the discovery of the life-history of the parasite. With regard to the other two, full accounts have recently appeared in the JOURNAL of the most outstanding new work. Tallquist's work on *Dibothriocephalus* anaemia was also discussed, and mention made of the fact that he had been able to isolate toxic substances from the worms, which when injected into animals produced severe anaemia. The diseases of animals which Dr. Nicoll selected for particular mention were onchocerciasis and grouse disease. He, however, had nothing to add to the accounts published in the JOURNAL in the course of last year. The paper was a particularly comprehensive and lucid account of what is apparently becoming an increasingly wide and complex subject. The Association is to be congratulated on having had such an able exponent, and it might be well that a greater number of authoritative critical reviews of this nature should find a place at the annual meeting. We venture to think that they would be more readily received and serve a more useful purpose than some of the more isolated and inconclusive researches to which we have to listen.

SECTION OF EDUCATION.

Mentally Defective Children.

Dr. F. C. SHRUBSALL put forward the report of a subcommittee, which made suggestions for the examination of mentally defective children. In the more obvious cases of mental defect the current system of examination scarcely admitted of radical change at present. For the more doubtful cases, on the other hand, the existing resources seemed entirely insufficient. To send a child to a school for defectives had very serious consequences for it and for its parents, and there was a possibility of the matter becoming still more momentous in connexion with eugenical measures. "Nothing could be satisfactory short of a systematic examination at properly equipped central institutions. Such institutions were further and urgently needed for the purpose of research, since one of the main lessons to be drawn from the previous investigations was the desirableness of extending them." And as one of the chief difficulties in carrying out such a plan would be to find the necessary scientific experts, it seemed eminently desirable to take some steps towards encouraging the study of psychology among medical men. Several universities had recently instituted a systematic course of training and an examination qualifying for a diploma in psychological medicine. But this training would be of little avail for the present purpose, unless its pedagogical no less than its psychological aspect were duly regarded.

ECONOMICS AND STATISTICAL SCIENCE.

The Reformatory Treatment of Inebriates.

Dr. DAVID HERON, a Perth student, Assistant Professor of Eugenics, University College, London, contributed a paper on the results of the reformatory treatment of inebriates. He gave particulars of the records of over 1,900 inebriates who had undergone reformatory treatment, and, although a number of them had only been at liberty for less than a year, only 331 could be said to be doing well or to have reformed. Even this small number of successful cases would have been considerably reduced had all the inebriates been under observation for periods long enough to test adequately the permanence of reform. In Scotland, where nearly all the inebriates discharged had been followed up, the inspector under the Inebriates Act stated that only 12 out of 181 could be said to be doing well. Turning to the experience of the Girgenti Inebriate Reformatory, formerly used for inebriates committed from Glasgow courts, he said that out of 29 who had completed their sentences only 2 were doing well after two years of liberty, while the number of convictions against 23 inebriates whose records were carefully followed totalled at least 165 after discharge. In dealing with the influence of the reformatories in reducing the amount of public drunkenness, Dr. Heron doubted whether the committal of inebriates in such small numbers, and for such limited periods, could have any appreciable effect, and declared that in studying the condition of these inebriates the one fact which dominated all others was the appalling amount of mental defect among them. All the available evidence went to show that the mental defect preceded, instead of being the result of, the alcoholism, that inebriety was more an incident in the life of the inebriate than the cause of his mental defect. For the inebriates themselves there was little hope of improvement. Even if they were brought under reformatory treatment at the earliest possible age, they would, for just as long as they were kept segregated, reduce *pro tanto* their fertility. They would not, however, except in a few cases, be restored to a normal measure of self-control. The only remedy for at least two-thirds of the existing extreme alcoholism was the permanent segregation of the mentally defective child from school age onwards. They must deal with the mental defect of which inebriety was only a sign, not in the police-court, not in the inebriate reformatory, but *ab initio* in the school for mentally defective children. Cut off the supply of the mentally defective at the source and the problem of extreme alcoholism, to the extent of at least two-thirds, would be solved in a generation.

Sir HENRY CUNYNGHAME said that in his official experience he had an opportunity of observing the work of reformatories for inebriates, and they might fairly say that, so far as reformative effect was concerned, nine-tenths of their work was completely thrown away, the real reason being that the persons treated were in certain respects mentally defective, and were irreformable. He had a strong impression that the religious influence was the right mode, and, while using all the influence they could to get these women segregated, they should use their influence with the various churches to sink their differences completely, so that they might devote their united efforts to such service.

The Congress of the Italian Society of Internal Medicine will be held at Rome in the second half of October under the Presidency of Professor Baccelli. The questions to be discussed are: Hepatic insufficiency and its treatment; internal secretions in relation to clinical facts; fever from the clinical point of view; morphology in relation to clinical facts.

MEDICAL INSPECTION OF SCHOOL CHILDREN.

THE Medico-Psychological Association of Great Britain and Ireland at its annual meeting adopted a report on this subject by a committee which was instructed to inquire into the propriety of the association framing and tendering to authorities advice on the search for and the definition of mental deficiency which is incompatible with the retention in elemenary schools, with suggestions for the appropriate treatment of such deficiency, and to consider any other cognate matters.

The committee at once recognized the fact that, as regards general principles, it was to a great extent following in the footsteps of the Royal Commission on the Feeble-minded, and that it was dealing with subjects which were receiving current attention in other quarters. Nevertheless the committee has endeavoured to treat independently all questions on their merits, and on these lines it has come to conclusions which support the general findings of the Commission. It has, however, endeavoured to consider details from the personal and varied experience of its members, especially with regard to the scientific recording and following up the progress of mental defectives. It attaches considerable importance to the formation of a complete *dossier* of each defective; and it thinks that for the better study of a case of mental disorder, even though at the time of school mental insufficiency might not have been apparent, it should be possible to refer to its school experience.

It is of the opinion also that closer relations between the experiences of the school and the asylum, through the co-operation of the officers of each, will be advantageous. It is possible that a warning as to the occurrence in a parent of those forms of insanity which by their nature and time of happening may threaten to bring about early evidence of hereditary defectiveness of mind in the offspring, may be of some service in modifying normal education where such may produce stress on a possibly deficient brain.

The committee is strongly of opinion that, in spite of any statements to the contrary, the provision of suitable means of training for children with low intellectual power, and the elimination of means of education which are not suitable to such children, may be found to be economically helpful. Experience shows that it is almost impossible to say beforehand that any child cannot be of some service, however slight, to the community. Such an opinion can only be formed after the capacity of the child has been practically tested for some period of time, and obviously such tests cannot be applied unless there is appropriate machinery in existence. On the other hand, experience proves that under scientific regulation, such as exists at Darenth, considerable saving can be made out of the work of those who, without that work, would be troublesome drones. The committee feels certain that it is wrong, socially and economically, to allow considerable masses of mental deficiency to lie deteriorating in institutions without being afforded opportunity for practical trials. This opinion applies not only to the unfortunate children, but also to the adult stages which they may reach later on.

The following recommendations and resolutions were adopted:

RECOMMENDATIONS AND RESOLUTIONS.

1. The committee, having seen the schedule of medical examination of children for mental defect, which appeared as an appendix to the annual report for 1909 of the Chief Medical Officer of the Board of Education, begs to endorse that schedule, suggesting certain amplifications (see Appendix I). The committee further considers that it is most desirable that authorities should arrange for facilities being provided for consultation in doubtful mental cases between school medical officers and recognized experts in mental disease, including the past and present medical superintendents of institutions for mental defectives.

2. That in all cases of mentally defective persons, provision should be made for a history of each case being kept as long as the case is under official supervision, and that records of the case should be preserved for reference. The committee further considers that the schedules of school medical inspection of every child should be preserved for future reference in case of mental failure at a later period of life.

3. When special provision has been made for the education of a mentally defective child under the Elementary Education (Defective and Epileptic Children) Act or otherwise, a report should be made to the local authority or other authority providing, by an expert, for the purpose of ascertaining the results, if any, of special treatment.

4. It is desirable that medical superintendents of county and borough asylums should be empowered to communicate with school medical officers about any cases under their care, whose children are attending elementary schools under the supervision of those officers.

5. The committee is of opinion that an essential feature in the solution of the mental defective problem should be that adequate arrangements be made for the permanent detention and control of mental defectives requiring such care.

6. The committee has no hesitation, after the inspection of Darenth, in expressing the opinion that the expense of the education and training of mentally defective persons, if properly conducted, is justified.

7. With regard to the suggestions for the appropriate treatment of juvenile deficiency, the committee has not been able to draw up any formal recommendation which would properly cover such an immense field of inquiry and at the same time be of practical use. The mere question of relative density of population renders it impossible to formulate any single proposition; so, too, does the difference in intelligence, natural habit of thought, environment, and anticipation of future calling and occupation which exists between urban and rural children; further, there has to be considered the obvious effect of variation in the extent of deficiency in any particular class.

The committee recognizes that, after all, the principal provisions for treatment must depend on the fitness, relative or absolute, of the defective for receiving modified education or training, independently of personal care and control, or the reverse. On such a footing appropriate provision will, with any necessary modifications, probably work out in the shape either of "special classes" or of institutional life, when by reason of their defect or of the unsatisfactory condition of home surroundings the special classes do not fulfil all requirements.

With regard to the special classes, in certain cases the only chance of justice being done to mentally defective children would appear to lie in co-operation between areas which cannot afford to make provision by themselves. In a large town of 160,000 inhabitants—Mannheim—a system is adopted which is set out in Appendix II.

With regard to the second line of treatment—the institutional—there is room for difference of opinion. But the committee puts forward (see Appendix III) a suggestion of a colony which it thinks has much to recommend it in convenience, economy, and benefit to the afflicted. It is quite recognized that all such propositions must be made subject to the details of the legislation which is so imperatively and universally demanded; but it is confidently thought that such legislation will not contain any provisions that will be inimical to the general idea.*

Though, of course, this suggestion is adaptable to the care of the feeble-minded in all classes of area, it gives chief consideration to the requirements of a rural county area. The principles must be the same everywhere; but there are differences between urban and rural areas which require different practical treatment. The comparative density of population is such a difference.

Since in certain cases continuity and permanence in supervision of mentally defectives is now admitted to be essential, the committee has rather gone beyond the childhood limit in its ideas, believing that economy and efficiency can be secured only by considerable concentration, and by avoidance of a break in that supervision.

The committee is of the opinion, also, that the existing institutions for idiots, such as Earlswood, the Royal Albert Asylum, etc., may be taken as models of institutions that will be required for such mental defectives as cannot be conveniently treated in colonies. Their present geographical distribution permits of the existing institutions becoming nuclei for certain districts, others being required

* Since this report was drafted it was found that the legislation proposed does not in any way interfere with the institution of colonies.

to fill up gaps, so that the whole of the kingdom should be supplied with such accommodation.

APPENDICES.

I.—*Schedule of Medical Examination of Children for Mental Defect.*

Note.—The object of the following schedule is to facilitate the investigation of suspected cases of mental defect. It is of a suggestive nature only, and is printed in the present form for the convenience of school medical officers making inquiries into the mental condition of feeble-minded children.

I. *Name of Child, Address, Name of School.*

II. *Particulars of Home Conditions, Environment, School Attendance, and other Factors.*

III. *Family History.*
Insanity, Feeble-mindedness, Alcoholism, Tuberculosis, Miscarriage, Syphilis, Epilepsy, other Characteristics.

IV. *Personal History.*
Constitutional Defects, Injury at Birth, Malnutrition, Rickets, Diseases of Childhood, Commencement of Teething.
Walking.
Speech, etc.
Physical State of Mother, Length of Gestation, Convulsions, Accident.

V. *Physical Conditions.*
(*a*) *General.*
Speech: Defective articulation.
Sight: Blindness, total or partial, errors of refraction.
Hearing: Deaf-mutism, partial deafness, partial mutism.
Nose and throat: Enlarged tonsils, adenoids, mouth breathing.
Control of spinal reflexes and of salivation.
(*b*) *Stigmata.*
General retardation—Cretinoid development.
Cranium—Microcephaly, hydrocephaly, asymmetry, rickets, imperfect closure of fontanelles, simple head measurement.
Hair—Double and triple vortices, wiry or supple.
Face—Irregularity of features.
Lower jaw—Protruding or receding.
Eyes—Mongoloid, presence of epicanthic fold.
Ears—Size, setting, conformation, lateral symmetry, size of lobes, attachment of lobe to the cheek, supernumerary lobules.
Tongue—Enlarged, furrowed, papillae enlarged.
Teeth—Irregular, absent, enlarged incisors.
Palate—Arched, narrow.
Fingers—Webbed, clubbed, defective in number or shape, supernumerary digits.
Limbs—Excessive length of upper limbs.

VI. *Mental Conditions.*
(*a*) *Reactions of Motor Mechanism.*
1. Formation of Motor Ideas. (Execution of simple and new movement from imitation.)
2. Storage of Motor Ideas. (Execution of simple familiar command by word of mouth.)
3. Power of control, initiative, purpose, and concentration. Success of motor output. (Execution of familiar complex movement.)
4. Motor Incompetence. Attitude in standing—Position of head, spine, and knees. Gait. Position of arms, hands, fingers, in horizontal extension. General balance.
5. Motor Instability. (Habits.) Rocking of body, Rubbing hands, Spitting, Biting nails, or Licking lips.
6. Motor Disturbance. Tremors (face, hand, tongue), Chorea, Epilepsy, Aphasia, Hemiplegia.

(*b*) *Reactions Resulting from Sensory Stimulation.*
1. Attention—Colour, shape, size, smell.
2. Formation of Memory Images:
(*a*) Recognition; objects, sounds.
(*b*) Recollection.
3. Association of Ideas.
4. Judgement (for example, length, size, distance).
5. Relationship (similarity, contrast, symbolism).
6. General concepts (possession, self-protection, purpose, concentration, initiative).

(*c*) *Emotional Conditions:* Interest, excitement, aggression, co-operation, affection, etc. (positive or negative phases).

(*d*) *Tests of Intelligence*—
1. Description of pictures, models, objects, familiar events.
2. Letters, words, reading (word blindness).
3. Counting, manipulation of simple numbers, simple money values.
4. Writing.
5. Manual Tests.

(*e*) *Tests of Will Power (under the above headings).*

VII. *Diagnosis.*—
(*a*) Physically defective—stating defect.
(*b*) Blind or partially blind.
(*c*) Deaf-mute or semi-mute or semi-deaf.
(*d*) Epileptic.
(*e*) Merely dull or backward.
(*f*) Mentally defective (Feeble-minded).
(*g*) Imbecile.
} *In this group, the symbols "a" to "g" are intended to be correlated when necessary.*

VIII. *Treatment recommended.*
(i) An ordinary public elementary school.
(*a*) Normal.
(*b*) Normal, but backward.
(ii) A special class for dull and backward children.
(iii) Special school (day or residential).
(*a*) Feeble-minded
(*b*) Moral defective
(*c*) Epileptic
} With notes as to after-care, custody, and the degree and character of manual training and ordinary school teaching likely to be advisable.
(iv) Unsuitable for special schools—
Imbecile, ineducable, invalid.

Note.—The above Schedule of Medical Examination was published by Sir George Newman, M.D., F.R.S.E., in his official report to the Board of Education for 1909, p. 208. The Committee have made several minor additions, with the full approval of Sir George Newman.

II. *The Mannheim System.*

The following is a short sketch of this system, communicated by one of the committee, Dr. Auden of Birmingham, who has studied it *in loco*.

The principal elementary schools correspond to what we should term normal schools. There are eight classes or grades, through one of which each child should pass each year between the ages of 6 and 14. Ninety per cent. of the children do so. For the brilliant scholars, various forms of secondary education are amply provided. If, however, at the end of a year, a child cannot satisfy the teachers that it is fit for promotion, it is relegated to another and parallel system of classes, the "coaching" or "repeaters" classes. Here during the next year it practically repeats the work of the class in which it failed to progress. It is important to note that, whereas in the chief classes the maximum teaching of children per class is 45, in the repeaters class the maximum is brought down to 35. If the child progresses sufficiently well he goes up a grade in this school each year. If he regains ground he is sent back to the chief classes, but will always be one year behind his class-age. The classes are divided, so as to allow of more individual attention. If, on the other hand, a child does not get on properly, it is sent to a third parallel series of classes, the "auxiliary" classes which are held in the same buildings as the "coaching" or "repeaters" classes. These, in Dr. Auden's opinion, are in strict conformity with the Special Classes established in England under the Defective Children's Act of 1899. Here the teaching is more concentrated still, the staff being as 1 to 20 as a maximum, but as a rule there are only fifteen children in a class, which is divided further into two divisions. At this point medical research comes into play, for each child's case and history are accurately taken, and it is very carefully examined by the School Medical Officer for any mental or bodily defects before it is sent to these classes; in doubtful cases the child is sent back to the coaching class. There are preparatory classes attached to this division, in which, as also in the lower classes, the teaching is on the kindergarten system. All through this series of classes the training is largely manual, the children attending the ordinary manual centre with the children from the normal classes. The children are retained in this series until 14 years of age, being carefully looked after as to food, clothing, etc., by a special committee. It is somewhat surprising, and certainly satisfactory, to read that the majority of those who pass through the auxiliary classes become self-supporting. There is yet another receptacle for defectives—the idiot asylum—for those who fail to come up to the standard of the last school. This is maintained by the State, and not by the education authority. The proportions of children in the schools are about 90 per cent. in the chief classes, 10 in the coaching classes, and 0.6 in the auxiliary. It will be noted that this system allows plenty of time for the child itself to demonstrate its capacity for learning, and for the discovery and correcting, if possible, of any physical causes of retardation; it avoids the wasting of slow or damaged brain power by vain endeavours to teach it with those of higher mental capacity, or, on the other hand, by throwing it on the rubbish heap through neglect; it obviates the waste of teachers' time and energy which depend much on having only one standard of brain to develop at one time; it concentrates teaching where it is most needed; and it is very comprehensive and thorough. The main classes are, as

a rule, held in the smaller schools of the six districts into which the town is divided for administrative purposes; the repeaters and auxiliary classes are held for the most part in two large two-department district schools. There is thus no stigma attached to these children, such as is found to attach in England to those who have been transferred to the "silly" or "balmy" school—a stigma which often sticks and prevents the children from obtaining work afterwards. At the same time the removal of the lower-grade mental defectives (as is contemplated in the 1899 Act in England, but is not strictly carried out) removes the chief objection of parents to send their children to a "special school." The cost is high, but the authority can pride itself on doing its best not only for the sound brain, but for strengthening and preserving that which is weak or damaged. The number of children receiving education in the Public Elementary Schools in Mannheim is about 26,000. The system depends upon a careful decentralization into complete educational districts. It has been put into practice in many large Continental towns, either in part, or entirely, for example, Bale, Brussels, Copenhagen, Stockholm, Vienna, and in many large German towns.

III. Plan of Colony System.

When, for reasons before stated, it is found that institutional life is required, the committee thinks that the central idea should be that of a colony in one or more selected places in an area. It thinks that there should be rigid adhesion to the principle that prospect of improvement by education and training in handicraft should be the essential qualification for admission. It thinks that the machinery of a colony should not be used under any circumstances for the purpose of detention solely.

The children may be divided into the following classes:

1. Mentally defective who are not epileptic.
2. Mentally defective who are epileptic.
3. Epileptic children who are either not at all defective mentally as far as educational purposes are concerned, or who are merely dull or backward.

The form of education will be of three types:

(1) Physical Training.
(2) Purely Manual and Industrial Training.
(3) General Mental Education.

The general arrangements will have to be framed to afford the education or training best suited to each class. Obviously, those epileptic children who are only excluded from the normal elementary school on account of their physical disease should not lose their right to general education, which may advantageously be combined with such manual training as will facilitate their becoming useful to a certain extent in after-life; on the other hand, the lowest class can only receive strictly manual training.

The fact of epilepsy complicates the whole position, for while many epileptics are much the same as other children except for the convulsions, there is potentiality of their being or becoming defectives of an undesirable nature, while they are often morally defective, and always are a source of harm to other feeble-minded children, who frequently are mimetically impressionable.

A word may be said here about moral imbeciles, who assuredly will have to be provided for somewhere. Assuming that radical improvement in the moral direction is not to be looked for, these still have to be taught, especially in the manual form. For this reason it would appear to be right to include them as scholars in the colony, since it would possess suitable means of giving such training. At the same time as many of them are criminals or quasi-criminals, their inclusion will entail a certain amount of provision for forcible detention, which may be a drawback to the general comfort. In any case they would have to be housed by themselves. By day they could mix under rigid supervision with the other children undergoing manual training.

To provide the manual and industrial instruction there should be a comprehensive set of workshops for such trades, etc., in which the children may be reasonably expected to acquire proficiency. A laundry, and possibly a needle-room, should be attached for training the girls.

As far as possible, the training of the children should commence on the kindergarten idea, and we think that the school should be staffed as far as possible by females, since they are found to be more apt and patient than men in such work.

For the adults such provision as can be seen at Darenth would appear to be suitable and sufficient. Some of the aged defectives might have manual occupation with the children, for which Darenth also supplies a precedent. Thus they would be taken away from the more vigorous life of the adult shops. A competent craft master should direct the work in the shops.

The adults, after being trained in the schools, may well be housed in cottages on the estate containing from ten to twenty each as may be found desirable. The houses for the men would be conducted by the training staff and their wives, while the nurses and teaching staff would take charge of the women.

The colony should be provided with sufficient land to afford training and occupation for those defectives whose condition, physical and mental, is suited to garden and farm work. Probably a larger number of defectives are capable of being usefully employed in simple outdoor work than in more technical handicrafts.

THE NON-OPERATIVE TREATMENT OF CANCER.

THE address of Professor CZERNY of Heidelberg at the meeting of German scientists and physicians in Münster is a carefully weighed and cautiously expressed message of warning to the public. He states that while a number of methods are undoubtedly capable of influencing the course of the disease, it is hopeless to apply any of them in advanced cases, in which secondary growths have appeared in the internal organs and marked cachexia has supervened. Before it is permissible to speak of cure cases must be watched for many years, and even then the determination of the value of a method of treatment must be made with great care, since the variability of the course renders statistical evidence of doubtful accuracy. In Germany upwards of 50,000 persons die each year of cancer, and if the facts that approximately one-third of the cases operated on remain free from recurrence, and, further, that the average duration of life after the onset of cancer is somewhere about two years, are taken into consideration, it is probable that at least 100,000 persons are suffering from cancer in Germany at any given time. The old principle still holds good, that the earlier and the more radically a commencing cancer is removed, provided that it is local, the greater is the chance that the cure will be lasting. In cancer of the face up to 80 per cent. and in mammary cancers up to 40 per cent. of cures are obtained in this way. The public does not hear of these results, because the surgeon does not advertise his successes; but whenever a failure occurs the news travels apace. Most surgeons know only too well that some forms of cancer nearly always recur after operation, and do not operate in these cases. But this is no ground for giving up operation altogether, as this would mean throwing away all that surgery has accomplished in the struggle against cancer. There is scarcely an experienced surgeon who is not able, after a practice of many years' duration, to record hundreds of permanent cures of cancer, clinically and histologically proved to be such.

In tracing the methods which have been adopted to treat cancer without operation, Czerny instances Busch's and Volkmann's production of erysipelas, Coley's fluid treatment, the various forms of "toxin" treatment introduced by Adamkiewicz, O. Schmidt, Doyen, Wlaöff, as well as tuberculin and crotalin treatment, as having failed to lead to striking results. The processes of haemorrhagic softening, necrobiosis, caseation, fatty degeneration of the cells and replacement by scar-like connective tissue, have been described as retrogressive changes, but Nature alone fails to clear the whole field, and the remains of the growth after the destructive processes have taken place only develop a more rapid overgrowth. It is therefore necessary to attempt to imitate and supplement Nature's efforts to effect a cure. After briefly referring to the experimental work of Hanau, Morau, Jensen, Ehrlich, Bashford, and others, he admits that the explanations offered for the failure in a percentage of cases to implant a tumour with success are not entirely satisfactory. Next he turns to the attempts to utilize immune substances in the treatment of cancer, and states that the employment of these substances has not effected any apparent result in man. In speaking of chemiotherapy he mentions the interesting experiments of von Wassermann and Keysser, who injected selenium-eosin into the tail veins of mice, and succeeded in causing

the tumours to break down. In a very few cases actual cure was obtained. The therapeutic dose of this mixture is, however, so close to the toxic dose that no one has dared to apply it to man. Neuberg and Caspari have reported excellent results with copper and zinc, and Gaube de Gers with colloidal copper. In Czerny's own institute R. Werner has employed cholin in the form of a combination of boron and cholin, and has been successful in following out an energetic action which the radio-active substance exercises on the tumour cells. Professor Czerny says it is too early to speak of the effect of this form of treatment in man at present, but he claims that it is useful in what he describes as the combined treatment. Passing on to other methods, he speaks of arsenic cautery pastes. Hebra employed Cosme's paste with success in lupus and facial cancer. Zeller is using a modification of this paste, and has obtained remarkable results not only in superficial skin cancers but also in breast cancers and even in cancer of the cervix. Here, again, it is too early to speak of cure. Czerny is not prepared to determine what part in the beneficial effect should be ascribed to the silicic acid of the Zeller treatment. Various forms of arsenic have been tried with varying degrees of benefit. Ehrlich has suggested that neo-salvarsan may prove more advantageous when locally applied in cancer than old salvarsan, as the former is soluble in neutral media and is easily diffusible. He passes over the so-called blood-purifying agents, such as guaiacum, salicin, formic acid, calcium, lecithin, and many others, with the remark that their beneficial effects are little apparent. He next discusses the light treatment, following up with x rays, radium, actinium, mesothorium, thorium x, and the like. The rays emitted from these preparations vary in their power of penetration, but attack young, rapidly growing cells in preference to other cells, destroy their nuclei, and by destroying the cells themselves cause the growths to disappear. But this form of treatment does not protect against recurrence. Czerny warns the operator against destroying in this manner too much tumour tissue at once, as the absorption of the products of disintegration may kill the patient. Fulguration and short-spark treatment are also dealt with, and it is pleaded for this method that in some cases the removal of nodules and remains after operation may be performed in this manner with advantage. At all events, he prefers the electric method to the destruction of tumour masses by carbonic acid snow.

Before concluding with a description of the combined treatment, which he regards as the best plan to work on, he makes some highly suggestive remarks on the duty of the State in regard to the cancer problem. Vast sums of money are spent on the combating of tuberculosis, leprosy, syphilis, and other diseases, but little is expended on the most terrible of all the diseases. It is necessary, in view of the large number of new suggestions which have been made for the treatment of cancer, to sift and study each medicament and method with care. This work can be adequately carried out only in a properly equipped institute. Dr. Czerny considers himself fortunate in having been placed at the head of such an institute in Heidelberg by the generosity of the German industrial firms, of rich individuals, and of the Government of Baden. If, he continues, the highly civilized nations—England and Germany—could be induced to build one Dreadnought less each and to invest the £2,000,000 in forty cancer institutes in each country, he is certain that the cancer problem would not only be solved within fifty years, but the number of those suffering from cancer would be reduced to one half.

The combined treatment, which has been applied to about 2,000 cases in hospital and about 1,000 out-patients, is described somewhat as follows. When no contra-indication exists against operative removal, the tumour is excised as radically as possible. In serious heart or vascular disease, in diabetes, nephritis, or in those tumours which recur almost constantly, the mass is cauterized by one or other method, or radiotherapeutic or chemiotherapeutic methods are substituted for the operation. In some cases dissemination is prevented by combining operation with fulguration, electric arc light, cauterization, diathermia, or x rays. The general condition is improved by giving Fowler's solution, sodium cacodylate, or some other arsenical preparation. Superficial cancers are healed by means of arsenical paste, electric cauterization or radiotherapeutic measures. In round or spindle-cell sarcoma with bad prognosis, an energetic radiotherapy is employed. In inoperable desperate cases a variety of the means previously enumerated must be cautiously applied, and one or other combination selected. X rays may be used locally, or radium or mesothorium may be tried. At the same time, thorium x or boron cholin may be injected either intravenously or subcutaneously or into the tumour. After two or three weeks' treatment a pause of one or two months is allowed to intervene, during which arsenic or silicic acid and general tonic treatment is given. Then the local or general treatment is again taken up. In each case it is necessary to individualize, and by careful selection of the remedies which may suit each patient considerable benefit may result.

REPORTS OF SANATORIUMS FOR TUBERCULOSIS.

King Edward VII Sanatorium.

The fifth annual report of this sanatorium, recently published, states that the increasing number of applicants for admission and the fact that the sanatorium is always full prove that the objects for which it was founded have been largely achieved. Like all institutions of a similar nature, there is too great a proportion of advanced cases among those admitted, but lately a strict examination of candidates has improved matters, and a fair proportion of the patients now belongs to the earlier stages. During the twelve months under review all the beds in the sanatorium were occupied continuously. During this period 271 patients were discharged from the institution. Of this number, 91 patients, or 33.5 per cent., came within the definition of "early cases"; 115 patients, or 42 per cent., were cases of moderately advanced disease; and 41, or 16 per cent., were suffering from the disease in its more advanced stages. The remaining 21 patients gave no positive evidence of pulmonary tuberculosis. The routine treatment prescribed for the patients during the period covered by the report followed very closely the lines detailed in previous reports. Tuberculin has not been included in the routine treatment, but was given to a few selected cases. The report, which is drawn up by the consulting staff, states that, with the value of tuberculin still a matter of controversy, it is well to have available reliable statistics with regard to the results of treatment of a large number of patients to whom no tuberculin was given. Such statistics are of distinct value as a "control." The number of patients treated and discharged since the institution was opened in 1906 until the end of June, 1911, was 1,270. Of this number only very few had a systematic course of tuberculin treatment. Treatment by vaccines prepared from organisms other than tubercle bacilli separated from the sputum proved disappointing. The results would suggest either that mixed infection plays a less important part in pulmonary tuberculosis than is often stated, or that it is very difficult to separate the particular organism which is responsible for the mixed infection. The ultimate results of 935 cases are as follows: Well, 431; alive, 122; dead, 283; and unknown, 99. From the statistical tables it appears that 160 males and 111 females were admitted during the year; the majority of cases—132—were from 20 to 30 years of age. The cases were divided into three groups, following the nomenclature of the International Tuberculosis Committee. Group I included those in which the disease was of slight severity, limited to small areas of one lobe. Group II, disease of slight severity, more extensive than Group I, but affecting at most the volume of one lobe, or severe disease, extending, at most, to the volume of one half lobe. Group III, all cases extending beyond Group II, and all with considerable cavities. In 54.9 per cent. of the cases in Group I the disease was arrested, in 34.1 per cent. much improved, and in 4.4 per cent. improved; in 6.6 per cent. it was stationary. In Group II the disease was arrested in 12.2 per cent., in 51.3 per cent. much improved; 19.1 per cent. were stationary, and 5.2 per cent. were worse. In Group III in none was the disease arrested, 20.5 per cent. were much improved, 35.4 per cent. were improved,

22.7 per cent. were stationary, 15.9 per cent. were worse, and 4.5 per cent. died.

From the laboratory report it appears that only a small proportion of specimens of sputum were examined, owing to the absence of the pathologist. In 74 patients examined 27 were found to have tubercle bacilli in their sputum, equal to 36.48 per cent., while in 47 cases the tubercle bacillus was not found. The report closes with notes of a visit the pathologist, Dr. Radcliffe, paid to some of the most important centres of bacteriological research on the Continent, and deals with the value of the treatment by tuberculin, with von Pirquet's reaction in relation to diagnosis, with the complement fixation reaction in tuberculosis, and with experimental work on the effect of digestion on tuberculin.

Adirondack Cottage Sanitarium.

The twenty-seventh report of the Adirondack Cottage Sanitarium (thus spelt by our friends across the water) has just been issued. This institution is situated at Saranac Lake, New York, in the St. Regis Lake region. One feature in connexion with this charity, mentioned in the report, is that it has no auxiliaries pledged to its support, no regular subscription list, no committee whose duty it is to raise funds; but it has always depended—and so far its confidence has not been misplaced—solely on the voluntary contributions of its friends, mostly visitors to the Adirondacks.

Dealing with the medical part of the report, we find, as is the case in this country, that fewer incipient cases have presented themselves for admission, and this, no doubt, is owing to the fact that a greater number of incipient cases first try to get well under home treatment, and it is only if this fails that they apply for admission to a sanatorium. Dealing with the system of classification, the cases under the heading "apparently cured" have diminished appreciably during the last two years. This, the report states, is not, as it would appear at first sight, due to a real falling off in the percentage of good results, but is no doubt principally owing to the much more stringent and searching laboratory methods which are applied in making the classification, such as centrifugalization of the expectoration, improved staining, and even cultural methods, and an increase in the number of tests made during the three whole months when the patient's secretions are required to be free from bacilli. Further, many patients from whose expectoration the bacillus has disappeared leave the institution before the end of the three months of observation required to place them in the apparently cured class have elapsed. Some notice must be taken of one feature of treatment which is of the utmost value to the patients. Every Sunday morning a talk is given to all new patients on the causes and nature of the disease, methods of guarding against infection, the principles of modern and institutional treatment, and what results may be fairly expected from treatment. On Monday morning the staff meets for general discussion of various problems arising as to individual patients, and to hear a brief paper by one of the members. Once a fortnight there is a meeting of the patients, at which a talk is given by a member of the staff on the fundamental principles of treatment, and the influence of fresh air, rest, exercise, and food are discussed and explained. This educative form of treatment is highly to be commended. Of the 304 cases treated at the sanatorium during the year, 106 remained at the end of the year. Of the 198 patients discharged, 27 were apparently cured, in 101 the disease was arrested, 42 were improved, 23 were unimproved, 2 died, and 3 were doubtful. Of 174 cases admitted, the expectoration of 81 contained bacilli, and in 93 it was not found; 155 patients were treated with tuberculin in conjunction with the usual open air and climatic plan, and in 95 of those reported on 13 were apparently cured, equal to 14 per cent.; in 48, or 51 per cent., the disease was arrested, 24, or 25 per cent., were improved, 8, or 8 per cent., were unimproved or failed, and 1 died. The medical staff is large, and examining physicians look after cases in New York, Baltimore, Philadelphia, and Boston.

As the result of the new Prussian law on cremation two crematories have recently been opened, one at Hagen in Westphalia, the other at Frankfort-on-Main.

LITERARY NOTES.

THE Cambridge University Press announces that eight new volumes in the *Cambridge Manuals of Science and Literature* will be published on October 1st. The series will then comprise fifty titles. Among the new volumes are *House Flies and How they Spread Disease*, by C. G. Hewitt, and *The Psychology of Insanity*, by Bernard Hart, M.D.

It will doubtless interest the many lovers of old books among our readers to learn that Messrs. W. and G. Foyle, the well-known booksellers, have found it necessary to take larger premises at 121–123, Charing Cross Road. Throughout the six floors in these buildings there are over 1,000,000 volumes arranged in strictly classified order. It is stated that at the time of the Crippen trial the treatise on homoeopathy that could not be found in any library or bookseller's, and on which practically depended the result of the trial, was obtained within a very short time from Foyle's, and the firm received a personal congratulatory letter from the Lord Chief Justice.

The Royal Academy of Medicine of Turin has published the terms of competition for the thirteenth award of the Riberi prize, which is of the value of £800 less Government tax. The period of competition extends to December 31st, 1916. All works offered must deal with medical science. They may either be in manuscript or printed; if printed they must have been published since 1911. They may be written in Italian, Latin, French, English, or German. Information as to the other conditions may be obtained from the General Secretary of the Academy, Dr. V. Oliva.

We are asked to state that copies of the *British Pharmaceutical Codex*, reviewed in the JOURNAL of January 13th, 1912, p. 95, can be obtained on approval for seven days by doctors in practice. Application should be made to the Pharmaceutical Press, 72, Great Russell Street, London, W.C.

We have received a copy of a *Class Catalogue of Current Serial Digests and Indexes of the Literature of Pure and Applied Science* exhibited at the meeting of the Library Association held at Liverpool on September 2nd and four following days. The *Catalogue* forms an appendix to a paper prepared for the meeting by Messrs. E. Wyndham Hulme and Charles Kinzbrunner. Its object is to indicate those periodicals which profess, by means of abstracts or index references, to record the output of the press in so far as it relates to their special provinces. We agree with the authors that such information, presented succinctly and revised periodically, should form a part of the bibliographical equipment of every librarian. We know of no existing publication which exactly covers this ground. The compilers say that "if a favourable reception is accorded to this experiment, it may be possible to arrange for the periodical publication of a handbook on similar lines, but comprising all branches of literature." We congratulate Messrs. Hulme and Kinzbrunner on their successful performance of a task which can only have been inspired by disinterested enthusiasm, and we hope that so useful an undertaking will receive the support which it deserves.

Among the contents of the third number of *Bedrock*, which will be published on October 1st, are Ancient Human Remains and their Bearing on the Antiquity of Man, by Professor Arthur Keith; and Modern Vitalism, by Hugh Elliot.

Dr. G. HAYDEN (Eltham, Kent) writes: In the BRITISH MEDICAL JOURNAL of this week in the "Literary Notes," you give some extracts from the work of a student who endeavoured to use the art of poetry as an aid to the study of medicine. Some twenty years ago at the London Hospital a fellow student also hit upon this artful device, and I venture to think with more success. At the present moment I can only call to memory his terse and dramatic description of malignant endocarditis; all the salient features are presented, and could be elaborated in the examination room. It was as follows:

> Pallor of face, and petechiae of skin,
> Terrible temperatures, rigors set in;
> Shifting heart murmurs, and optic neuritis,
> Typhoid's not in it, it's endocarditis.

British Medical Journal.

SATURDAY, SEPTEMBER 21st, 1912.

ISOLATION HOSPITALS.

The Local Government Board in England has issued an important report[1] by Dr. H. Franklin Parsons on isolation hospitals. It takes the form of a supplement to the report of the medical officer of the Board for 1910-11, and an introduction is contributed by him. The inquiry upon which the report is based was carried on by Dr. Parsons from time to time as opportunities occurred during his official connexion with the Board, and has been completed by him since his retirement. The primary objects of the inquiry were to investigate the complaints received by the Board concerning the large and increasing cost of construction of isolation hospitals, and to furnish instances of satisfactory hospitals which have been erected at a reasonable cost. Dr. Parsons has been assisted in his inquiry by Mr. B. T. Kitchin, F.R.I.B.A., architect to the Board. The report is described as a supplement to that *On the Use and Influence of Hospitals for Infectious Diseases*, written by the late Sir R. Thorne Thorne, first issued in 1882 and reprinted in 1900. The new report, however, seems really to replace rather than to supplement the earlier, and will, there can be no doubt, be of the greatest value and assistance to local health authorities.

The increased cost is shown to be in part due to increase in the cost of labour and building materials, and a greater desire for efficiency; but the fact that the cost of recent isolation hospitals has varied from £300 to £500, and even more when calculated in relation to the number of beds provided, shows that there must have been room for economy in many cases. It is, however, admitted that in some cases the excessive cost has been caused by the high price paid for the site, due in many instances to the objections of local residents, and in others to the heavy expense involved in preparing the site for building. Dr. Parsons lays it down that an isolation hospital should be provided in every populous district for the reception of infectious cases, at least for such as are without proper lodging and accommodation, or which occur under circumstances involving special danger to the public health, and in particular for the reception of the earliest cases in an outbreak. He points out, however, that, by combination of small districts to make the necessary provision in a single building, important economies may be effected, especially in the preparation of the site and in the water supply and drainage; the classification of patients according to the diseases from which they may be suffering is facilitated, and it becomes easier to maintain a competent administrative and nursing staff. Dr. Parsons gives special attention to the consideration of what is conveniently called "cross infection." The term embraces cases in which a patient may be admitted suffering from two specific diseases or from secondary infections, as in scarlet fever or measles, and also the conveyance of specific infection from one patient to another. On this account it is necessary to make adequate provision, whatever the size of the hospital, for separating patients, but what adequate provision means varies in different affections. In those of what Dr. Parsons calls "short-striking distance," a space between beds sufficient to prevent direct contact and the transference of droplets of mucus in coughing, together with careful aseptic nursing may suffice, but in infections of a more diffusible character complete aërial separation is necessary. It has been sought to obtain this end in more than one way. It may be assumed that every isolation hospital ought to consist of two or more ward blocks, and these blocks appear to be of two principal types, which are illustrated by block plans prepared by Mr. Kitchin. In the one the wards each contain four or more beds, and are connected together by enclosed communication. This is the most economical and convenient plan for the simultaneous treatment of a number of cases of the same disease. In the other form, each ward contains one or two beds only, and is separately entered from the open air. A hospital on this plan costs more per bed to erect, and involves more work in attendance, but owing to the facility it affords for classification of patients a given number of beds can be used for a greater variety of cases than where the wards are larger, and it is much easier to deal with cases in which the diagnosis is still doubtful. The plan of dividing the larger wards into cubicles by glass partitions, which was, we believe, first adopted in one of the children's hospitals in Paris, is mentioned, and it is pointed out that, while it presents considerable advantages in respect of isolation, it demands a high standard of nursing with scrupulous attention to detail.

The use of what are commonly called temporary materials, such as galvanized iron lined with matchboard, or wood or iron lined with asbestos, is discussed, but, apart from the risk of destruction by fire or storm, it is pointed out that the cheapness of such buildings as compared with permanent hospitals is not in reality so great as may at first sight appear. The cost can be properly compared only when the expenditure on site, water supply, drainage, furniture, and other requisites are included, and this may easily amount to more than the building itself. Further, though this no doubt is a remediable objection, in the temporary buildings supplied by makers of such constructions, the bed-space allowed is less than the Board's standard, and they are commonly very deficient in means for the classification of patients and in accommodation for the staff. In this connexion it may be noticed that it is recommended that the minimum cubic space in wards of the ordinary type should be reduced from 2,000 to 1,872 cub. ft. per bed, with a floor space of 144 sq. ft., and a wall space of 12 ft. for each patient, as at present. This would allow a pavilion containing two rows of beds to be 24 ft. instead of 26 ft. wide. It is added that in properly ventilated cubicles and single-bed wards, in which the patient is completely separated from any other, a further reduction in the number of cubic feet might be allowed.

Dr. Parsons devotes one chapter of his report to a discussion of the utility of isolation hospitals in preventing the spread of disease. In this connexion he discusses the suggestions first put forward some twenty or more years ago by the late Mr. Brendon Curgenven, and more recently advocated in papers published in the Journal by Dr. Milne, as the result

[1] Report on Isolation Hospitals. By H. Franklin Parsons, M.D. London: Wyman and Sons; Edinburgh: Oliver and Boyd; Dublin: E. Ponsonby. (Cd. 6342. Price 1s. 7d.) To be obtained through any bookseller.

of his experience at Dr. Barnardo's Homes in connexion with scarlet fever. His treatment is described in the report as the rubbing in of pure eucalyptus oil over the whole body twice a day during the first four days, and afterwards once a day until the tenth day of the disease, and the swabbing of the tonsils and pharynx with carbolic oil (1 in 10) every two hours for the first twenty-four hours. Dr. Parsons points out that Dr. Milne lays it down that this treatment must be applied at the very commencement of the illness, a condition which it may be possible to fulfil in an institution such as a school or orphanage, but not commonly possible in the homes of the poor, where medical assistance may not be sought until the disease is established and others probably infected. The method was tested by Dr. Biernacki at the Plaistow Hospital for a year in every case of scarlet fever. It was found that cross-infection became commoner, that an unusual number of probationers contracted scarlet fever in the course of their duties, and that a larger proportion of doubtful cases of scarlet fever developed the disease after going to the convalescent home. In addition, though this may have been due to coincidence, the percentage of return cases rose above the average. It is noted, however, that a modified system of antiseptic nursing has been found by Drs. Rundle and Burton to give good results at the Liverpool City Hospital, Fazakerley.

Isolation hospitals will, however, in any case continue to be required for patients who do not enjoy proper accommodation at home, or in instances in which the case occurs under circumstances involving special danger or alarm to the public. There is, therefore, no near prospect that local authorities will find it unnecessary to be provided with hospital accommodation for the isolation of infectious diseases. Among other reasons mentioned for this conclusion is the occurrence of unrecognized or unnotified cases. Such cases are apt to be numerous at the present time when the type of scarlet fever is mild, and are a frequent source of infection at school and elsewhere. It also seems now to be established that scarlet fever is much more infectious at an early stage than used to be supposed. On the vexed question of "return cases" the conclusions are that they cannot be obviated by prolonged detention in hospital, and that the infection appears to persist in mucous discharges from the nostrils, throat, or other organs, rather than in the desquamating epidermis.

With regard to small-pox, Dr. Parsons is emphatic as to the value of isolation in a suitably situated hospital in preventing the spread of the disease. Owing to the active and far-reaching nature of the infection and the persistence with which it clings to places and articles of clothing and bedding, a case of small-pox kept at home is a greater danger to susceptible persons in a house or neighbourhood than one of scarlet fever or diphtheria, and the increasing proportion of the population who are unprotected by vaccination makes it the more important to place a patient suffering from small-pox where he will be surrounded only by a cordon of efficiently vaccinated persons. Small-pox hospitals must be situated at a safe distance from populous areas if they are to be effectual in preventing the spread of the disease, and may therefore be very costly, though Dr. Parsons considers that they may be used at times when not likely to be required for their primary purpose as sanatoriums for consumptives or as convalescent homes for scarlet fever patients. If, however, a small-pox hospital be used for either of these purposes it is essential to make arrangements beforehand by which it can be immediately cleared of patients when it is necessary that it shall revert to its special purpose.

Coming back to the question of cost of isolation hospitals for other infectious diseases, Dr. Parsons points out that, as compared with ordinary general hospitals, the cost per bed of an isolation hospital must as a rule be high because the number of beds provided is small in relation to the cost of the site and the work necessary in preparing it, because of the need for the classification of patients according to sex and disease which involves a multiplication of wards, and because since the patients are acutely ill a comparatively large staff must be provided. The excessive cost of some isolation hospitals is attributed to the institution being too small, a defect to be remedied by combination of small districts, to the selection of an unsuitable site involving large expense in the preparation and making of roads, the supply of water and the disposal of sewage and the making of foundations, and last, though less frequently, the want of regard to economy in design or insufficient supervision and control over the cost of erection.

We have been able to give only a general outline of the report, which will be carefully studied by medical officers of health called upon to advise on the erection and administration of isolation hospitals; but we may say that, incidentally, it contains much information which will be of use to medical men who are consulted with regard to the erection of cottage hospitals; the conditions, especially with regard to isolation of individual patients, are different and less exacting, but on the general question of choice of site, its preparation and the economical erection of convenient buildings, this report affords many useful hints.

PSYCHOTHERAPY IN ORGANIC DISEASE.

As has often been pointed out in this JOURNAL mental or spiritual treatment often cures functional disease. At the present day we hear much about it under various names. The giving of new names to old things is one of the features of modern faddism. Everything from the fasting cure to faith healing of the most unorthodox kind goes back to the dimmest dates in human history. This is a point that should never be lost sight of, for the food faddists are always telling us that in their particular form of nutrition, whether it be nuts or grated cheese, lies the whole secret of health, and this in face of the fact that the vegetarians of the East are the prey of as many diseases as the lovers of the flesh-pots in the most luxurious capitals of Europe.

Professor James J. Walsh, who dealt with the subject in the *Albany Medical Annals* for January last, tells us of a special school of mental healers at Poughkeepsie, one of whose professors succeeded in getting a hearing from the United States Senate because of the wonderful new scientific powers he had discovered and controlled. Naturally it is always the "cured" cases that are appealed to, and there was any number of "cured" cases, so that Congressmen became persuaded that here was something worthy of investigation. Unfortunately, nothing is said about the result. Walsh considers that such men must have a wonderful personal magnetism, as shown in their power to persuade people to their views. "The only effect produced, of course, is through the mind and the influence it can exert upon the body. But it is startling to note how many patients that have made the rounds of the regular practitioners of medicine are cured in this way." Here it may be

asked, Cured of what? Cured, we say, of sensations of pain and other subjective symptoms of diseases which are certainly to a large extent imaginary. It would be a great mistake, however, to assume that because they are imaginary they were not a real affliction to the patient. This is the point on which many members of the medical profession go astray and lose hold of patients who would be quite amenable to cure by ordinary medicine with a large infusion of faith, the latter being the more active ingredient. Until the profession learns that faith healing has a real and definite place in therapeutics, it must be content to see a vast number of people in this neurotic age go to spiritualist quacks of one kind or other.

Dr. Walsh's thesis, however, is the part of psychotherapy in organic disease. We have never ourselves gone further than to contend that some of the symptoms of organic disease could be removed by faith healing. For example, although a truly malignant growth cannot be removed by faith, it may in rare cases disappear of itself. Most of us have had experience of this kind of thing in the case of innocent growths such as warts. Faith may move mountains, but, according to all trustworthy experience, it cannot remove a cancer. Still, the soothing of pain, the lessening of inflammation, and the inspiring of hope which generates a feeling of well-being in the patient, are effects of a very real kind not to be despised by the scientific practitioner. Dr. Walsh points out that there are so many people suffering from what was at the beginning a true physical ailment but which afterwards is only a group of symptoms made inveterate by the failure of the patients to take exercise, to eat and get air and sleep enough, that it is not hard to find improvement of symptoms as soon as they can persuade themselves that they ought to be better. In Professor Walsh's experience, organic disease can be distinctly modified by mental influences. This result is seen in affections that have no relation to the nervous system. He cites the case of a patient who suffered for some months from what was thought to be indigestion, and who was then found to have a tumour in the region of the stomach. Cancer was diagnosed, and an exploratory incision made, the patient being told that what was really intended was to attempt to remove the growth. It was found that the conditions would not allow radical operation, and the wound was stitched up. He was told that now an operation had been done he ought to be better. It was intended to tell him later the real state of the case. During convalescence appetite returned; he began to take more interest in life, lost the sense of discouragement, and feeling that whatever discomfort he felt could be referred to the operation scar, he began to put on flesh, and in six months he gained over 30 lb. Of course he ultimately died of his cancer, but he had been given many months of vigorous active life and a sense of well-being that made the operation eminently worth while. Walsh believes there are cases on record in which patients have gained 40, 50, and even 60 lb. in like circumstances. These are good examples of the influence of the mind over the body, and its power to overcome even the depressing effects of cancer.

In constitutional diseases dependent on obscure organic lesions striking examples of mental influence are sometimes seen. Walsh says that Graves's disease can probably be more influenced by strong suggestion than by any remedies that we have. He thinks that even pernicious anaemia is probably more benefited by getting the patient into a favourable frame of mind than by any other means. In tuberculosis the state of the patient's mind is important. One who faces the outlook bravely may live in spite of organic ravages. Dr. Walsh says: "Our three greatest specialists in tuberculosis in this country [America] were all given up as hopelessly affected by the disease in their earliest career. Yet all have done fine work." In heart affections of all kinds, not only cardiac neuroses but organic heart affections, the patient's state of mind is, he says, of more importance than any drug. The mind acts through the circulation, closing or relaxing vessels all over the body, and so relieving the burden thrown on the heart. In diabetes and gout the mental state is very important. Drugs are helpful in all these conditions, but behind them is the action of the mind gathering hope from their administration.

Walsh pleads for the deliberate use of psychotherapy, which has been unconsciously used with wonderfully good effect. Avicenna was right when he said that the presence of a physician often does more good than all his remedies. Mondeville, surgeon to Philip the Fair, thought cheerfulness so important a factor towards recovery that he suggested the employment of a jester to visit surgical patients when they were depressed. Dr. Walsh finds it an eminently wise practice of the Middle Ages that a patient should be asked to confess and receive the sacraments before the doctor saw him, for if this was suggested afterwards the patient might believe that there was no hope. This rather recalls Dame Quickly when she sought to comfort Falstaff on his deathbed by saying "a' should not think of God," adding the hope that "there was no need to trouble himself with any such thoughts yet." We may say, however, that we have more than once known a devout believer, whose state seemed almost desperate, begin to recover as soon as he had been comforted by the last rites of the Church. His mind was at peace and the mental state reacted on the body.

Psychotherapy has its place in medicine, and a very prominent place; but psychotherapy alone, except in certain well-defined cases of nerve disorder, can do little or nothing. If psychotherapy unaided could do all that Dr. Walsh claims it can do, there would be no need either for the physician or for the druggist, and the Christian Scientist and his congeners would have the field to themselves. It is not psychotherapy that could have brought about the revolution in surgery with which Lister's name is associated, nor could psychotherapy have effected what has been done in the prevention of contagious diseases. It is quite true that in some diseases there is a natural tendency to get well, and this is the foundation of much apparent success not only of quackery but of rational medicine. But it is not less true that there are many cases in which, as Gull said, Nature is trying to kill the man, and here the specifics come in. The action of these may be aided by mental healing, but such healing will not kill a malarial plasmodium in the blood or rid the system of the infection of a transmissible disease. When we are told that patients must not be allowed to cherish certain forebodings with regard to the course of their disease, nor to keep turning over in their minds the dread of developments that may come, we can only say that this is true enough; but it reminds us of the advice to a needy patient not to worry, but to eat a beefsteak and drink a glass or two of generous port every day. There is no class of patient more given to this kind of foreboding than doctors themselves. Hence they usually are the worst of all patients. They either know the complications that may occur, or eagerly read up the subject. We are the most unfortunate

of all people, for we know that a tiny sore or an almost imperceptible wart may be like the little cloud, no bigger than a man's hand, which overspread the heavens. To live under the shadow of a great fear deprives a man of energies that might otherwise be available for curative purposes. In dispelling this shadow psychotherapy may play a most useful part. In its employment, however, there are two factors, the doctor and the patient. The former must be a person who can influence, and the latter one apt to respond to such influence. Hence the self-centred neurotic is the subject best adapted for psychotherapy.

SANATORIUM BENEFIT IN MANCHESTER.

THE sanatorium scheme for Manchester, an outline of which has already been given in the JOURNAL, is evidently to meet with strong opposition from the Joint Committee of the Divisions of the British Medical Association, and from the four Provisional Local Medical Committees, which are all united in their opinions. It is a remarkable circumstance that so far only the general outlines of the scheme have been divulged, and that it has not been found possible to obtain any adequate and authentic information as to details. The Joint Committee of the Divisions has in consequence taken the precaution of sending copies of the decisions of the Annual Representative Meeting as to the administration of sanatorium benefit to both the Sanitary Committee and the Provisional Insurance Committee, with a statement that the local profession regards it as of fundamental importance that the scheme should conform to those decisions. A request has also been made that the Sanitary Committee should receive a deputation from the Joint Committee, and a promise has been given that this will be arranged at an early date.

There can be no doubt that the scheme as it stands at present fails in several important points to accord with the cardinal principles of the British Medical Association. With regard to the administration of sanatorium benefit, among other objections, it is especially urged that it will tend to deprive general practitioners of many of their private patients suffering from tuberculosis, and will thus be a serious menace to private practice; and, further, that it will make the tuberculosis dispensary into little more than an enlarged consumption out-patient department conducted by the hospital staff, with all the objections to out-patient departments aggravated by the fact that if patients refuse to attend the dispensary they may be denied sanatorium benefit altogether.

In the annual report of the medical officer of health a special chapter is devoted to the position of Manchester with regard to tuberculosis. Dr. Niven says it is evident, judging from the number of notifications, that the estimate of the number of cases in the city must be revised, as it cannot be less than 5,000 and may be more if, as is thought to be possible, a number of cases are not being notified. The available beds at the disposal of Manchester are put down at 543—that is, 424 under the guardians, 69 at Clayton Vale Hospital, and about 50 at the Crossley Sanatorium; in addition, there are 114 beds available for surgical tuberculosis. In both cases the demand is far greater. Dr. Niven considers that the Insurance Committee will be forced at no distant period to extend sanatorium benefit to the dependants of the insured, on the ground that it will be found difficult, if not impossible, to grant the benefit to an insured person while withholding it from his wife and children. It is expected that the City Council will have to provide a considerable sum of money. In all probability the Insurance Committee will apply, under Section 17 of the Act, to the City Council and the Treasury to make up any expenditure in excess of the sum provided by the Act; but there is this difference, that the Treasury, in so far as it is liable at all, is liable only for half the excess expenditure on the treatment of insured persons and their dependants, while the City Council is liable for the whole of the remaining expenditure on both the insured and the non-insured, and as insured persons can exert a very great influence at elections, severe pressure will be applied to the council to provide all the necessary treatment. Moreover, of the total cases, the Poor Law authorities have always from 400 to 500 cases under treatment, and must treat at least 1,000 cases a year; as soon, however, as sickness benefit falls due, many persons who now receive treatment under the Poor Law will be able to dispense in large measure with assistance from the guardians, and will require sanatorium benefit from the insurance fund instead, and this it will not be easy to refuse. This will undoubtedly save the poor rates, but the necessary provision will have to be made by the public health authority, as the Act forbids arrangements with Poor Law authorities. Of the annual sum of 1s. 3d. per insured person taken from the insurance contributions for sanatorium benefit—amounting to about £880,000 for the whole country—it is estimated that about £14,000 will be available for insured persons belonging to Manchester, and the City Council may also receive a sum estimated at about £24,000 as a grant for capital expenditure out of the provision made by the Finance Act.

The death-rate from tuberculosis in Manchester is higher than in any other great English town. During 1911 there were 1,116 deaths from phthisis, giving a death-rate of 1.56. Bad as this is, it is an improvement on the average of the last ten years, which was 1.78. The death-rate from tuberculosis other than phthisis was 0.52; this is also an improvement on the average of the last ten years, which was 0.64. It has been stated that a high proportion of all children are tuberculous, and that while many recover, many others remain tuberculous, and the opinion has been adopted and acted upon in France that if children in the incipient stages are treated in suitable institutions, they can be freed from the disease and fortified against it. Following on these lines, the Manchester Education Authority has been presented with an estate, and is equipping a school for the treatment of incipient cases. Dr. Niven, however, expresses his conviction that the great majority of phthisical persons, especially among the poorest classes, contract the disease after childhood, and even when the disease has been present in childhood and been followed by recovery, the cause of an attack in adult life is not resuscitation but reinfection. He again urges, as in previous years, that a special fund should be established, out of which money should be advanced to families in suitable cases to enable them to occupy suitable houses and procure good food and clothing, subject to the sanitary authority being satisfied that hygienic precautions are carried out and the money wisely expended. He concludes the special chapter with some remarks which have caused much concern to general practitioners: "As regards Manchester, it is to be observed that the housing of the citizens will not lend itself generally to the use of shelters, and that henceforward, as hitherto, it will be found necessary to treat cases largely away from their own homes—at all events, for a time. Indeed, when sickness benefit

accrues, the demand will no doubt become imperative. It will be found difficult, if not impossible in most cases, to give sanatorium treatment at home in the sense of a carefully regulated life with a system of graduated labour and close observation of symptoms. Hence the majority of cases will require treatment in a sanatorium—for a time, at all events."

THE MEDICAL BENEFIT REGULATIONS.

THE draft provisional regulations made by the Joint Committee jointly with the Insurance Commissioners and the Scottish and Welsh Insurance Commissioners as to the administration of medical benefit are to be considered at a meeting of the Advisory Committees at the offices of the Civil Service Commission, London, to-day, Friday, September 20th. The draft has already, we understand, been considered, and in its general outline approved by the Ministers most directly concerned. It was also considered at preliminary meetings of the medical members who have retained their seats on the Advisory Committees, which were held on Wednesday and Thursday, September 18th and 19th. While affording alternative methods of remuneration on a capitation basis, and on the system of payment for each attendance as well as a combination of these systems, recognizing the necessity for paying special fees for extras, and dealing with the question of mileage, the draft does not, we understand, specify the rates of payment. The Government proposals in this respect are, we believe, not likely to be made known before the meetings of the Cabinet which will be held early next month. It is no doubt to the meeting of the Advisory Committee of September 20th that the chairman of the West Riding Insurance Committee referred last week when he spoke of an important meeting to be held between the Insurance Commissioners and Advisory Committee of the Government and the medical men. The statement has been widely circulated in the press, as well as the anticipation that some arrangement would then be entered into. It will be seen that the chairman of the West Riding Insurance Committee had somewhat misinterpreted the information he had received with regard to the nature of the meeting, and his expectation that an arrangement would then be come to is obviously due to this misapprehension. No arrangements on behalf of the medical profession can be made by the medical men who still remain on the Advisory Committee. When the draft provisional regulations are promulgated—and we presume that this will be done shortly after the meeting on September 20th—they will be considered by the State Sickness Insurance Committee, and subsequently the Central Council of the Association will make a report to the Divisions with a view to instructions being given to their Representatives at the special Representative Meeting, which will no doubt be summoned at as early a date as the progress of business permits.

MEDICAL SERVICE ON EMIGRANT SHIPS.

AT the present moment a large tide of emigration is flowing out of England. While Canada is obtaining her share of emigrants, Australia is clamouring for as many as she can get; the ships ordinarily engaged in the passenger trade between the home country and the antipodes are not capable of supplying the continuous demand for berths, and extra ships have been called for. Without attempting a detailed definition of the term "emigrant," it may be stated generally that all steerage passengers, or to give the more euphemistic title, "third class," sailing in a foreign-bound vessel from British ports come under the category of emigrants. As such, the ship carrying them becomes, *de facto*, an emigrant vessel subject, in addition to those applicable to ordinary foreign-going passenger ships, to the special regulations of Part III of the Merchant Shipping Acts. That these regulations should be all-sufficing is almost too much to expect of any legislative measure, more especially when it is remembered that the majority of the sections were drawn up to meet the requirements of emigrants in the old days of sailing clippers. The ordinary liner belonging to established firms, engaged in a regular run and carrying about 600 third-class passengers or emigrants in addition to a large complement of saloon and second class, is not much affected by the present boom in emigration, further than in having a full complement in the steerage on every outward voyage instead of numbers fluctuating according to season. The majority of emigrants are being carried in vessels purely of the cargo-boat type, temporarily fitted—according to legal specifications of course—for the carriage of emigrants on the outward passage. Homeward bound they revert to their original function. It is with this type of ship that seagoing members of the medical profession are especially concerned in view of the sudden increased demand for ship surgeons. A ship of this class will carry between 1,000 and 1,100 "statute adults." A "statute adult" may be either one individual or two children between the ages of one and twelve years, infants in arms are not reckoned. The size of the crew is regulated in the deck and victualling departments by the number of "statute adults" carried, while that of the engine room does not vary in regard to the number of passengers. Thus a ship of this class may have from 1,400 to 1,500 souls on board for a period of six weeks in continually changing climatic conditions. As the ships do not carry passengers on the homeward voyage, the law does not require a surgeon to be on board unless the crew numbers over one hundred souls. Thus the companies prefer engaging as surgeons medical men who will take their discharge in the colonies. In other words, surgeons are simply passage workers, with or without some remuneration for their services. While, as yet, there is no legal compulsion for these ships to carry more than one surgeon, the majority carry an assistant surgeon. A section of the Merchant Shipping Acts "recommends" that a second medical officer be carried in the event of there being more than 1,300 "statute adult" passengers. Thus, of these two medical men, both may be, and one as a rule is, a passage worker. The passage worker system applies also to the stewards, the majority of whom are first voyagers, with not the slightest knowledge of their duties or the ways of ships. Under this system permanent officials of the ship have great difficulty in maintaining proper discipline on board, as the men have one object—to get out to the colonies. According to law, the surgeon of an emigrant vessel is an important official, specifically mentioned in the sections of the Merchant Shipping Acts and vested with powers which are almost paramount, should he consider it necessary to enforce them. His responsibilities and work are far greater than in ordinary ships owing to the large numbers of this particular class of passenger he has to deal with. The average incidence of disease and demand for medical attendance are far greater among a thousand steerage passengers during a period of six weeks in varying climates than would be the case with, say, 6,000 for one week only. The strain, too, is much more prolonged in the event of any virulent epidemic disease breaking out. Also in these ships there is no private practice, well remunerated or otherwise. Finally, this class of vessel is nothing but a makeshift compared with an ordinary passenger vessel, and has none of the comforts, accommodation, and living conditions of a liner. In return for service under these circumstances a medical man may be offered either £10 a month, with possibly a bonus of one month's pay if everything has been satisfactory—from the owner's standpoint—or simply a passage in a cargo boat with all the drawbacks incidental

thereto. There is a good deal to be said for "passage working" on both sides, in so far as the discharge of the surgeon at the end of the outward passage is concerned. It is often a convenience to both parties, although it necessarily restricts the sphere of the professional ship surgeon, who finds so many ships closed to him thereby. But six weeks' service under conditions as outlined above, either as principal or assistant medical officer, are worth more than £10 a month or a passage to the steamship owner, who presumably is not engaging in a profitless enterprise. A bonus of ninepence a head is a little below the rate granted by the Government for National Insurance, and would not be too much remuneration for services rendered by the surgeons of an emigrant vessel during a period of six weeks among what is practically a class of unselected lives. There is also no reason why a man who only wants to go out to the colonies should not receive the average rate of monthly pay given to ship surgeons in addition to the bonus suggested as some compensation for services rendered, especially amid the discomforts and drawbacks of a cargo ship. It remains, as in other instances, entirely with members of the medical profession, either as permanent or as temporary ships surgeons, to demand adequate remuneration for the work they perform. They should bear in mind also that working a passage in a converted cargo boat is a very different matter in every way from doing so in a ship carrying saloon passengers. Shipowners are business men, and cannot be expected to drive any but business bargains. Medical men are in duty bound to their profession to do likewise.

THE COLON BACILLUS IN WATER SUPPLIES.

In another part of this issue will be found a report of the discussion which took place during the Liverpool meeting on the varieties and significance of *B. coli* in water supplies. It was introduced by Dr. A. C. Houston, of the Metropolitan Water Board, who, as will be seen, illustrated his remarks by a large number of tables and diagrams. They condense into small space an extensive knowledge of this and other aspects of water-protection work, and represent an amount of hard-won experience scarcely to be gauged by others than those who study the figures in the light of personal experience of laboratory investigations. The form which the discussion took, perhaps, makes it caviare to the general, but the vital importance of the question to the community will hardly be denied. Every one alike must feel an instinctive desire that the water he drinks shall be entirely pure. On the point of what constitutes entire purity there is perhaps room for difference of opinion, and there is certainly no consensus as to how far it is justifiable to condemn water as unpotable on the ground of the presence of *B. coli* therein. Taking the extremists on either side, the one party may be represented as claiming that if any *B. coli* whatever can be found in a sample, the supply from which it is derived should at once stand condemned. The other points to the fact that the *B. coli* is not a pathogonic organism in itself, and is not necessarily an indication of pollution from human sources, and further claims that even when such pollution is known to exist, the history of certain localities shows that water-borne disorders, such as typhoid fever, do not necessarily occur. The extremists, however, are not numerous, the vast majority of authorities accepting the view that the presence of *B. coli*, whencesoever derived, is evidence of pollution of the water at a comparatively recent date by matter of the sewerage class, and that this being the case, the chief practical questions concern what tests of its presence are most reliable, and what amount of any given water must be examined for *B. coli* in vain before it can be passed as sound. In regard to the latter point, Dr. Houston suggested many years ago some half-dozen *B. coli* ratios on which a water might be written down as of first, second, third, or other quality. Nevertheless, at Liverpool he urged, not for the first time, that though the higher of these standards of purity as regards *B. coli* contents should be the aim, the final verdict on a water should be based on a summation of geological, topographical, physical and chemical, as well as bacteriological considerations. As the meeting thought sufficiently well of this view to formulate it in a resolution, it may be anticipated that before long it will gain general acceptance. Meantime, it is satisfactory to know that Dr. Houston does not preach for the benefit of other communities what he does not himself practise for that of Londoners. Despite the not altogether desirable sources from which is derived the water issuing from the domestic tap of London—or possibly on account thereof—Dr. Houston maintains in his laboratory a very high standard, and manages to contrive that the purifying measures adopted under his general supervision at the reservoirs shall almost invariably bring up to it all water allowed to be distributed through the mains. On this fact the Metropolitan Water Board and its customers are alike to be congratulated.

THE ANTIQUITY OF MAN.

THE address on the antiquity of man (p. 669) which Dr. Keith delivered before the British Associatian at Dundee, affords such a review of the present state of knowledge as only one who is a thorough master of the subject could have attempted. In spite of the many recent additions, both of fact and theory, in which Professor Keith has taken no small share, the subject is still enshrouded in many mysteries. If we peer into the dim horizon of the geological past, when the world was young, we feel at once the need for some other measure of time than years, or even centuries. Even when we measure by geological epochs, there is room for much difference of opinion in attempting to estimate how long ago it was that man first walked the earth. It is now easy to follow modern man back through the age of metals to the neolithic period, a relatively short epoch by geological measure. Early in that epoch not only did man exist, but man of the modern type. It is on the question how much further we should go back in trying to fix the date of his more primitive ancestors that difference of opinion becomes acute, and sometimes, as occurred, we believe, in Dundee, leads to discussion characterized by more vehemence of expression than the calm which should characterize scientific men would appear to warrant. Even Professor Boyd Dawkins holds that for the early stage of the evolution of man we must go back to the Pleistocene period, which the best geological opinion appears to hold lasted some 400,000 years. Other geologists of eminence hold this to be a too conservative estimate, and the thousands of coliths which have been collected by Mr. Benjamin Harrison in Kent, seem now to be generally accepted as worked flints dating from the Pliocene, which preceded the Pleistocene period. Professor Keith's address is closely reasoned and should be read in the original text, but we may here point out that he stated three lines of argument of a biological rather than of a geological nature, which if accepted seem to make it necessary to put the beginning of man a very long way back in geological time. Dr. Keith considers that modern human races—white and yellow, red, black, or brown—are all so similar in structure and constitution that they must be supposed to have arisen from a common stock, and that the nearest approach to this common stock is the aboriginal Australian. Now investigations in Egypt have fairly well established that there is no definite evidence of the evolution of a higher or more specialized type during the six thousand years which have elapsed since the men lived whose bones occur in the

earliest burying place. Secondly, it is known that the great anthropoids made their appearance long after the gibbons, that both stocks survive at the present day, and Dr. Keith thinks it probable that the human stock became differentiated at or about the same time as the great anthropoids, so that it may be supposed that a very primitive form of man came into existence during the Miocene period, that which preceded the Pliocene period. We may thus have to go back from two to three million years for the beginning of the human stock. The third argument has reference to the appearance and disappearance of the Neanderthal man. Accepting, then, the view that a creature with human characters was evolved near the beginning of the Pliocene period, Professor Keith arrives at an estimate of an antiquity for man of at least a million and a half years.

THE EPIPHANIN REACTION IN SYPHILIS.

Weichardt's epiphanin reaction has chiefly been employed for tuberculosis and diphtheria. A few experiments have been carried out with syphilitic serums. Owing to the minute care which must be bestowed on the details of the test in order to obtain reliable results, it does not appear likely that it will attain wide popularity for clinical purposes, but it is of importance, especially in the study of the nature of the antigen-antibody reaction. The reaction—or, rather, test—is a manifestation of the difference of surface tension of a fluid containing both antigen and antibody reacting on one another, and that of a fluid containing both antigen and antibody, but not acting during the time when the indicator records the surface phenomenon. There is no doubt as to the ingenuity of the process. One tube is made up with a definite quantity of serum, a definite quantity of alcoholic syphilitic extract of liver, a definite quantity of baryta water, a definite quantity of normal sulphuric acid, and a definite quantity of phenolphthalein, to which some strontium chloride has been added. The same quantities are used in every tube. A second tube has water instead of serum, a third water instead of extract, and a fourth water instead of serum and extract. Thus tubes 1 and 4, when mixed, contain exactly the same constituents in the same quantities as 2 and 3 when mixed. But since the antigen (extract) and the antibody (serum) are allowed to interact before the indicator determines the degree of alkalinity in tube number 1, the mixing of 1 and 4 will yield a different result to the mixing of 2 and 3. Dr. Fritz M. Meyer, working under Professor Lesser,[1] has stated that when the serum is used in too high concentration the results may be erroneous, since the colloids may inhibit a positive reaction unless so much diluted that they do not affect the surface reaction, or the salts may give a positive reaction in high concentration, which does not depend on the presence of antibody. He has taken every possible precaution, and in testing 100 syphilitic and other serums has plotted out his results as curves according to the concentration of the serum. These results were compared with the results of Wassermann's test. In all save one, in which the Wassermann was positive, the epiphanin test was also positive. The exception gave a doubtful result. In those cases in which the Wassermann gave a negative, and the epiphanin test gave a positive reaction, the serum was derived from patients with latent syphilis and secondary stage syphilis. The doubtful cases in which the epiphanin test was positive, and the Wasserman test negative, were soft sore, alopecia, ozaena, disseminated choroiditis and abortions. There were 18 control serums, of which 1 case of acne and 5 others gave a positive result; these included carcinoma and infectious diseases. Meyer points out that the curve in these cases differs so essentially from the curve of the syphilitic case that confusion is impossible. An explanation has yet to be sought for these positive reactions with non-syphilitic serum. Absolute specificity probably does not exist in connexion with these bio-chemical products. In any case, the chemico-physical phenomena on which the epiphanin test depends are so delicate that we may safely expect illumination from them in the still dark processes of antigen-antibody reaction.

[1] *Berl. klin. Woch.*, February 12th, 1912

SCHOOL HOURS

A discussion has been going on in the *Cape Argus* for some little time past with regard to the organization of work in Cape Town schools. The school day at present lasts, in the case of ordinary schools, five hours, and in the case of high schools six and a quarter hours, the day beginning at 9 o'clock in both cases and ending at 2 and 3.15 respectively. In both cases there is a five minutes break between 9 and 12, and a further break of thirty-five minutes at 12.15, during which the children are supposed to take their mid-day meal. If the practice of educational authorities elsewhere is to be taken as a criterion, it can hardly be contended that the Cape Town school day is too long. The children in the ordinary schools are only at work for four hours and twenty minutes, and even in the high schools for only five and a half hours; whereas in public elementary schools in London the lower forms are supposed to be at work for five hours, whilst those in the upper forms, whose ages average between 12 and 14 years, work for five hours and a half. Whether the time is properly divided at Cape Town is quite another question. A mid-day interval so brief as thirty-five minutes is certainly very unusual. In London the corresponding interval is a full two hours. Such an interval in the day's work allows plenty of time for a meal and perhaps a little exercise, and in all ordinary individuals, whether children or adults, must suffice to relieve any extra brain tension created by the morning's occupation. This seems a sensible arrangement, for though many business men who live more or less constantly on their nerves find they get through their work more comfortably if they cut their mid-day interval down to half an hour or so, it cannot be good for growing children to be forced or encouraged to adopt a like course. One would expect them in the long run to suffer physically, and their power of attention to ebb so rapidly that the afternoon's work would practically be wasted time. Indeed, thirty-five minutes at mid-day is so curious an interval as to suggest its origin in an attempt to compromise between the desire of the masters not to let the length of each day's tuition fall much below the European standard, and the wish of Cape Town parents to have their children at their own disposition during the greater part of the afternoon. But the true remedy would seem to lie, not in unduly abbreviating the mid-day interval, but in commencing work earlier. The hour of 8 o'clock in the morning could hardly be too early in so excellent a climate as that which Cape Town is so fortunate as to enjoy, and the children could then be sent home at 11 o'clock for their mid-day meal, getting back at 1 for their afternoon work.

TABES AND PAINLESS CHILDBIRTH.

The relation of tabes to pregnancy and childbirth is an interesting subject which has been discussed both by obstetricians and neurologists. The latter, Dufour and Cottenot more especially, have noted how easily incipient tabes may be overlooked in pregnant women. Nervous ladies may walk oddly, pains in the abdomen are very common in the course of sexual life, and incoercible vomiting is a symptom which the obstetrician cannot overlook. But there is a well-known complication of the disease in question—gastric crises—and it is not surprising to find that it has been misinterpreted more than once in the case of gravid tabetic patients. One very important symptom usually marks the coexistence of tabes and parturition, namely, the absence of the normal "pains" associated

with the expulsive contractions of the uterus. Fruhinsholz and Rémy[1] have recently reported an instructive instance of the complication. A woman, aged 36, was admitted into the maternity at Nancy because a midwife whom she had consulted the day before on account of a bloody discharge told her that she was actually in labour. She had slept soundly all through the pains on the night before admission. The os was well dilated, the uterine contractions quite distinct and frequent, yet the patient felt nothing. It was difficult to get her to bear down, for this labour was her fourth and she knew what "pains" meant, as all the previous deliveries were painful. Not until the second stage was well advanced did she feel sure that she was giving birth to a child. At length she was delivered of a fine female child weighing 3,200 grams, or over 7 lb. The placenta came away painlessly, and weighed over 1 lb. The child was weaned when 2 months old. It was, when the report was published, 16 months old and remarkably well developed. The history and after-history of the mother were carefully reviewed. The first and second labours were normal. The third ended in the expulsion of a macerated fetus at the seventh month. The patient had fallen downstairs a fortnight previous to the premature labour, but she noted earlier in the pregnancy a widely diffused rash accompanied with headache. A few years later the patient suffered from incontinence of urine and then from gastric crises, both misinterpreted. The last pregnancy was associated with the most distressing vomiting, preceded by violent crampy pains radiating from the epigastrium to the loins and shoulders. Bloody mucus, but rarely food, was ejected during the crises. Lastly, delivery, as has been explained, was painless. The after-history left no doubt as to the nature of the complication. During the puerperium Romberg's and Westphal's symptoms were marked. The sight was failing and the crises continued. There was incontinence of urine. The patient's condition was unpromising when the report was published, whilst the child, 16 months old, showed no sign of hereditary syphilis. Abadie and other obstetricians have observed another symptom associated with tabes in pregnant women, quite the reverse of painless labour. Painful uterine crises occur simulating labour long before parturition takes place. Tabes is, indeed, remarkable for its mimicry of symptoms common in other disorders.

[1] *Annales de gynéc. et d'obstét.*, March, 1912, p. 144.

LIP READING AND ITS REVELATIONS.

A SERIES of detective stories of a novel kind is now appearing in the *Strand Magazine*. In them are recounted the remarkable experiences of Miss Judith Lee, a clever young lady who has acquired an extraordinary proficiency in lip reading, and who uses her talent for the unveiling of conspiracies and the prevention of crime. But it is evident that if the art of lip reading were common we should almost be living in the fabled Palace of Truth, and we should have to take refuge in what the historical French policeman, in the approved official style, called *un système de mutisme complet*. The cinematograph by itself is sufficiently indiscreet; but, if the audience had the power of Miss Judith Lee, the revelations of lip reading would often be extremely disconcerting. In a paper on the subject which recently appeared in the *Revue de Psychologie*, Mdlle. Attilia de Perreti tells the following story: At a cinematographic exhibition a surgical operation in all its details was realistically shown. As the film was rolled out the patient was seen stretched on the table. Then the surgeon came on the scene, knife in hand, ready to open the abdomen. His face was stony in its coolness. Not a muscle quivered. Only a slight movement of the lips was perceptible; this seemed to indicate some sternly controlled emotion. There happened, however, to be among the spectators a number of pupils from an institution for the deaf and dumb, and while the other people were thrilled with the fascinating horror of the sight, the children burst out into loud laughter. They had read the meaning of the movements of the surgeon's lips; the supposed twitches of nervous tension were interpreted by the deaf-mutes, who read that he was humming to himself the refrain:

> On va lui percer le flanc.
> Rantanplan, plan, plan!
> Tireli, ranplan!

Lest this should be taken as a proof of callousness by any of those malicious persons who lose no opportunity of traducing the profession, we think it well to place beside it a story of a very great soldier, who has never been accused of inhumanity. It is related that on the morning after a battle in one of our Egyptian campaigns, Lord Wolseley was heard entering the compartment of his tent which he used as an office. He seemed, as was natural after a victory, to be in good spirits, and approaching his working table he took up a paper, singing while he read it, to the tune of "So early in the morning":

> List of killed and wounded,
> List of killed and wounded,
> List of killed and wounded,
> Ti tum, ti tum, ti tay!

To return to lip reading, Dr. Bérillon, wishing to inquire into the truth of the story of the cinematograph, invited M. Baguer, Director of the Seine Institution for Deaf-Mutes, to read a paper on lip reading before the Société de Psychothérapie. M. Baguer, by way of experiment, sent a number of adult deaf-mute women to a cinematographic exhibition. The result was conclusive; many of them could read words, phrases, and whole sentences on the lips of the actors. The moral which surgeons who lend themselves to such displays should take to heart is, if their lips move, to be careful of their language.

PROTECTION FOR QUACK REMEDIES.

IT is stated by the *Australasian Medical Gazette* of August 10th that in the Federal House of Representatives the attention of the Minister of Customs was drawn to a statement by Dr. Burnett Ham that an analysis of an advertised cure for cancer showed that it consisted of 24.43 per cent. of starch (bread), 75.57 per cent. of water, and 4¼ grains of borax. The advertised price was 1s. 6d. for a 2-oz. bottle, and the label described it as "A remedy for the world. Cure for blood poisoning, pleurisy, pneumonia, erysipelas, all inflammations, typhoid and other fevers, and cancer in its various forms." Asked if he would take steps to prevent the sale of such a compound, Mr. Tudor replied that if the article was an imported one the department would take steps to prevent its introduction into the Commonwealth, but if it was manufactured in Australia no action could be taken until the people gave the Government the necessary power. The Federal Government could not prevent the manufacture and sale of such goods in any State, but the Department of Trade and Customs had prevented thousands of such quack medicines coming into Australia during the last two or three years. It is something to have quack remedies of foreign manufacture kept out of any country, but the Minister's statement seems to suggest that this may be done only for the protection of home produce.

SIR WILLIAM TURNER, K.C.B., Principal and Vice-Chancellor of the University of Edinburgh, has been nominated by the German Emperor a Knight of the Royal Prussian Order pour le Mérite in the section of science, as a recognition of his eminence as an anatomist and anthropologist. The number of members of the order is strictly limited, and the vacancy thus filled was created by the death of Lord Lister.

England and Wales.

[FROM OUR SPECIAL CORRESPONDENTS.]

SOUTH WALES AND MONMOUTHSHIRE.

King Edward VII Hospital, Cardiff.

A serious result of the Insurance Act was announced at the last board of management meeting of this hospital, when it was intimated that the employees of no less than forty-two firms had written to say that, in consequence of the demands under the Act, they would be unable to continue their subscriptions to the hospital. A circular stating this had been sent to the subscribers, and in it the fact was mentioned that practically the whole of the patients in hospital could be treated only in hospital, and that there was no treatment suitable provided under the Act. The chairman, Colonel Bruce Vaughan, said that, as it was, the hospital needed another £3,000 per annum before the whole of the beds could be opened, and, with a waiting list of 914, and the possibility of the subscriptions being materially reduced, it would presently be necessary to consider the propriety of closing some of the beds. He appealed to the workmen to support an institution which provided them with treatment which the Insurance Act could not give them. Some of the workmen members of committee said they had not heard of the proposed stopping of contributions, but the Taff Vale Railway representative said that to many railway servants the National Insurance Act was the last straw, and out of £1 a week a man had not even a penny to spare. Another member suggested that a scheme similar to that in vogue at Leicester should be adopted—namely, a contribution of 1d. a week from the workmen. Colonel Vaughan hoped that this would be done, for such a scheme would allow them to open all the beds, and to provide a convalescent home as well.

Swansea Hospital.

A special meeting of the board of management was held last week to consider the circular sent by the honorary medical staff to the effect that, with one exception, they would not, except in urgent cases, attend at the hospital any insured persons except on terms suitable to the profession.

Mr. Hughes, of the Dockers' Union, said he believed that the doctors were serious, and that this circular was an informal resignation. In any case it meant that 90 per cent. of the patients attending the hospital could not be treated. He admired the trades union spirit of the medical profession, but he looked upon their suggested action as syndicalism run riot. Mr. Aeron Thomas agreed with Mr. Hughes that this weapon of the doctors was two-edged and would cut in many ways that they did not think of. Passive resistance paled by the side of it. Mr. Brook, speaking for the medical staff, said that they were absolutely unanimous and perfectly serious. What all the speakers had overlooked was the scandal which the medical profession wished to stop, and which for years they had been unable to put an end to—namely, the continuance of underpaid contract work. It was no answer to say that there were any number of applicants for such posts when advertised. It was decided to defer further consideration of the circular until November 27th.

MANCHESTER AND DISTRICT.

The Early Notification of Births Act.

In spite of strong opposition from the medical profession, the Manchester City Council has adopted the Early Notification of Births Act, although little more than a year ago it refused to do so. Several weeks ago a deputation from the Joint Committee of the Divisions waited on the Infant Life Preservation Subcommittee of the Manchester Sanitary Committee, and laid before it in no doubtful terms the objections to the Act. The deputation urged: (1) That notification by a medical attendant would involve a breach of professional confidence, which, especially in matters relating to sex, ought to be held inviolable. (2) That the duty of notification, if required at all, ought to be imposed on the parents, not on the medical attendant. (3) That it was unjust to impose this or any other public duty on medical men without a fee, which, however, in the present case would not remove the first objection. (4) That cases of confinement attended by medical men were not cases in which any good could be derived from early notification, as the patients were under the personal care of the medical man attending, and visits by health visitors would not be required, and would, in fact, be resented. The deputation further said that there was ground for fear that if the Act were adopted the resentment of the profession might be carried to the extent of refusing at any cost to notify. The deputation was informed that the Sanitary Committee fully appreciated that medical men ought to be paid for notifying, and it had gone so far as to approach the Local Government Board with a request that the corporation might be allowed to pay a fee, but as the Act itself made no provision, the permission was not granted. On the whole, the subcommittee professed that, while wishing to show every possible consideration for medical practitioners, it was not convinced that the objections of the profession outweighed the advantages which were expected to be derived from the Act, and the committee reaffirmed its recommendation to the City Council, but put forward a number of suggestions for the working of the Act which it was hoped would lessen the objections of the profession. The subcommittee was, however, definitely informed that the suggestions would not satisfy the profession. Steps were next taken to memorialize the City Council, and a circular letter setting forth the objections stated above was sent to every member of the Council. At the meeting of the Council on September 4th there was only a comparatively small attendance of members owing to the holiday season, but in closing a long debate the chairman of the Infant Life Preservation Subcommittee said that the Act was in operation throughout London and in 295 sanitary areas containing half the population of England and Wales, that 59 of the 75 county boroughs had adopted it, and that Manchester was the only one of the leading towns where its provisions were not applied. The Council then decided by 44 votes to 41 to adopt the Act. A meeting of the Joint Committee of the Divisions has since been held, and the greatest indignation is expressed with the decision. It is stated that the Local Government Board is considering whether the time has not come when the Act should be made compulsory throughout the country.

Sanatorium Benefit in Salford.

The Salford scheme for sanatorium benefit, which has received the sanction of the Health Committee and the Provisional Insurance Committee, differs in some important respects from the Manchester scheme, referred to elsewhere, but none the less is being strongly opposed by the local Medical Committee. Out of a population of about 230,000 there were, during the year 1911, in Salford 380 deaths from phthisis, which is about the same as the average of the last ten years, and 196 from tuberculosis other than phthisis, which is higher than in any previous year except 1905, when the deaths numbered 206. Thus for both phthisis and other tuberculous diseases the death-rates, 1.6 and 0.8 respectively, are slightly higher than even in Manchester, where the rates are among the highest in the kingdom. The Salford scheme provides—

(*a*) That the medical officer of health shall be the chief tuberculosis officer, but for administrative purposes only.

(*b*) That a part-time specialist in tuberculosis shall be appointed to give two half-days a week, at a salary of £130 a year, with complete and independent control of the clinical work.

(*c*) That two assistant whole-time medical officers shall be appointed at £300 a year each, to act under the part-time specialist.

(*d*) That two nurses shall be appointed at £90 a year each.

(*e*) That a tuberculosis dispensary shall be established by the Health Committee in a shop belonging to the corporation in Regent Road. The dispensary is to be specially open on two evenings a week to suit the working classes, and the tuberculin treatment will be given where necessary, with medicines and appliances.

(*f*) That the clerical work shall be done for the present by the staff of the Health Committee.

(*g*) That the small-pox hospital at Drinkwater Park shall be fitted up as a temporary hospital for fifty patients.

(*h*) That the five beds at the Crosley Sanatorium, which are now retained by the Health Committee, shall be at the

disposal of the Joint Committee of the Health and Insurance Committees.

(*i*) That the cost of the hospital and sanatorium beds shall be divided between the Health and the Insurance Committees in proportion to the number of cases sent by each.

(*j*) That domiciliary treatment shall be carried out by the general practitioners of the borough on the following scale of fees: Report, 5s.; visits or consultations at surgery, 2s. 6d.; injection of vaccine, 2s. 6d.; vaccines to be at the cost of the administrative authority.

(*k*) That every insured person who applies for sanatorium benefit shall be referred to the dispensary for examination, and his case then submitted to the Insurance Committee, which will decide whether he is to receive home, dispensary, hospital, or sanatorium treatment.

Beyond these particulars, which have been published, the Provisional Medical Committee has been unable to obtain any further information. The resolutions of the Annual Representative Meeting were sent to the Health and Insurance Committees, but the local profession has not been consulted in any way, and as details were definitely refused when application for them was made, the Provisional Medical Committee has declined to meet the local authorities to discuss the question, and has decided to appeal directly to the Local Government Board to refuse to sanction the scheme. The objections put forward by the Provisional Medical Committee are: (1) That the medical officer of health cannot in addition to his ordinary duties have time for the purely administrative functions of a chief tuberculosis officer, and even if he had the time, the clinical work ought not to be subordinated to an administrative officer, as is the evident intention. Moreover, it is said that general practitioners will never tolerate control by an officer who from the nature of his office cannot have a personal and practical experience of the modern treatment of tuberculosis. (2) That the device of a part-time specialist for two half-days a week is quite inadequate even as a temporary expedient. (3) That the recommendation of the Departmental Committee on Tuberculosis, that the work of the dispensary should be as far as possible conducted by general practitioners, is ignored, as it is evidently intended that the whole routine work of the dispensary shall be done by the two assistant whole-time officers. This arrangement is not likely to enlist the sympathy of the public; it would deprive general practitioners of many of their present patients, and it is an insult to suggest that the medical men of Salford are incapable of carrying out the work of a tuberculin dispensary. (4) The financial provision for domiciliary treatment appears, as far as can be ascertained, to be inadequate, and suggests that it is intended to give as little domiciliary treatment as possible and to insist on dispensary treatment, making the dispensary practically into an ordinary consumption out-patient department. The Provisional Medical Committee finally states that it has various suggestions to offer which it would be glad to forward if the Local Government Board desires.

MACCLESFIELD INFIRMARY'S APPEAL TO THE GOVERNMENT.

The Board of Governors of the Macclesfield Infirmary is evidently convinced that the conflict between the medical profession and the Government is not a mere local struggle, but one which will have to be settled on a national basis. At a recent meeting of the board a letter was read from Dr. J. B. Hughes on behalf of the medical staff, calling attention to the pledge of the British Medical Association which has been signed by most of the practitioners of Macclesfield, and which may interfere with the work of the hospital unless satisfactory arrangements are made. A member of the board pointed out that the dispute was not between the infirmary governors and the medical staff, but a general question between the profession of the whole country and the Government. That being so, it was hardly a matter for reference to the House Committee, as had been suggested, and it would be useless for the committee to discuss the subject with the medical staff. Accordingly a resolution was unanimously carried that the board should make an appeal to the Chancellor of the Exchequer declaring that it was to the interest of the infirmary and similar institutions and of the medical treatment of the sick poor that the Chancellor should come to terms with the medical profession.

WEST YORKSHIRE.

SANATORIUM BENEFIT AT HALIFAX.

AN unsatisfactory situation has arisen at Halifax in connexion with the administration of sanatorium benefit under the Insurance Act. A few weeks ago, on the invitation of the Sanatorium Subcommittee of the local Insurance Committee, a deputation from the Executive Committee of the Division attended a meeting of the Sanatorium Subcommittee. It appears now that the temporary scheme had already been settled without conference with the local profession, and all that was required from the deputation had reference to the question of fees for domiciliary attendance and other services. The spokesman of the deputation went further than this and communicated to the subcommittee trenchant criticism of the scheme, such criticism having been carefully drafted by the Executive of the Division at a time when it was not thought the scheme had gone so far towards actual adoption. The medical officer of health, taking exception to certain statements made on behalf of the deputation, immediately sent in his resignation as a member of the British Medical Association to the central office. The subcommittee as a whole was so far favourably impressed by the deputation as to agree that it would be advantageous for the local profession to be represented on it, and the deputation undertook to secure the nomination of two local practitioners. A meeting of the Division was subsequently summoned, at which there was a large attendance; this meeting unanimously endorsed and approved the action of the deputation and the Executive, and elected two members to serve on the Sanatorium Subcommittee. Coincidentally a meeting of the local Insurance Committee was taking place.

The chairman of the Sanatorium Subcommittee, who is also chairman of the Borough Public Health Committee, and who, like the medical officer of health, seems to have been annoyed by the criticisms of the deputation, strongly urged the full committee not to elect the representatives of the profession to the subcommittee, because they would not serve on the full committee, and this course was adopted. There can be no valid reason why the local profession should thus have been excluded. Members are co-opted on Education Subcommittees who are not members of town councils, and there are also, among other precedents, committees of boards of guardians. The action of the chairman of the subcommittee in urging the exclusion of medical representatives may have been inspired by the free and frank criticism of the deputation, but it is certainly not in the best interests of the public and of sufferers from tuberculosis.

Scotland.

[FROM OUR SPECIAL CORRESPONDENTS.]

DEFECTIVE CHILDREN IN EDINBURGH.

THE Council on Child Welfare has issued four reports from the Standing Committee for Edinburgh and Leith, with recommendations, on defective children, infant supervision, and juvenile delinquency. Regarding defective children in Edinburgh, the report states that from

the facts put before the Committee they are of opinion that, although in Edinburgh a great deal is being done for defective children, both by statutory bodies and charitable agencies, a great deal more still requires to be done. There are at present 146 mentally, and 114 physically, defective children in the two special day schools of the School Board and in three special centres; and 58 in the School for Skin Diseases. Upwards of 50 blind, deaf, and dumb children have been placed in institutions by the Board under the Act of 1890. The Edinburgh Parish Council maintains about 50 imbecile children in Baldovan and Larbert institutions. No record is kept by the Council of defective children among children boarded out. As yet no exhaustive or systematic medical inspection has been made of defective children of school age in Edinburgh. With regard to defective children under school age and above a year old practically nothing is known.

The committee recommends the establishment of certified industrial schools for mentally and physically defective children, of a farm home for the feeble-minded over school age, and legislation for the feeble-minded.

The report on infant supervision contains a series of recommendations relating to the milk supply and other matters regarding child life.

Pulmonary Phthisis and Forres Hospital.

At a special meeting of the managers of the Forres Hospital held on September 10th the question of the admission of patients suffering from pulmonary phthisis was discussed. Dr. Adam moved that such patients should not be admitted, as there was no room for them. Their admission would make the present buildings useless for other purposes. If any epidemic occurred only eight beds would be available instead of sixteen, and that would not be ample provision. Dr. Rennie, in seconding the motion, said he supported the views of Dr. Adam; he saw no other way than the provision of a special place for such cases. The motion was unanimously adopted, the chairman remarking that after such a decided expression of opinion from the doctors it was the duty of the managers to write to the district Insurance Committee and say that they could not see their way to admit consumptive patients.

The Purification of the Air of Glasgow.

By means of numerous experiments, lectures, and demonstrations the Corporation of Glasgow has endeavoured to educate the citizen to the necessity of an unpolluted atmosphere in the interests of the physical well-being of the community. It has also given him an opportunity of assisting in the crusade against smoke by supplying domestic gas-cooking appliances to householders free of charge. Special arrangements have also been made for the supply of gas and electric-heating apparatus, and two years ago a smoke abatement exhibition was held, which was highly successful. In the three weeks during which it was open 80,000 visitors viewed the exhibits, and a surplus of £1,050 was realized. Another exhibition on similar educational lines is to be opened in the city, and the work will be maintained until October 12th. An influential committee is sparing no effort to make it instructive and interesting, and the school board has undertaken to co-operate. There will be sixty-six exhibitors, representing the leading firms in the country, and all the most modern electric and gas appliances for lighting, heating, and cooking will be on view. Lectures and demonstrations on the advantage of these appliances over coal fires with their smoke pollution of the air will be given regularly.

Ireland.

[FROM OUR SPECIAL CORRESPONDENTS.]

Salaries of Dispensary Medical Officers.

At the last meeting of the Enniscorthy Board of Guardians, a committee appointed to examine the application of the medical officers of the union for a graded scale of salaries reported that any increase of salary would come out of the rates, and that under the National Insurance Act the duties of the medical officers would be considerably decreased. The committee, therefore, did not recommend any change from the present system. This report was unanimously adopted.

Post-Graduate Classes in Dublin.

Post-graduate courses will commence in Dublin, both in connexion with Trinity College and also with the College of Surgeons, on September 23rd. In the Trinity College course the clinical instruction is undertaken by a lecturer in medicine and another in surgery, whereas in the College of Surgeons course the clinical instruction is given by various medical men at practically all the clinical hospitals in Dublin. Through the kindness of the board of Trinity College arrangements have been made by which members of the course may reside in college and dine on commons in the college hall. Each course will last three weeks.

Shelter for Street Waifs.

Last week a camp for homeless boys, erected under the auspices of the Women's National Health Association, was opened at the site of the old Ormond Market, Dublin. For some time past this area has been in a state of ruin, the houses having been condemned by the corporation as unfit for human habitation several years ago. Over 2,500 square yards have been cleared, and a circular camp in which there are twelve cubicles has been built out of the remains of the former houses. There is sleeping accommodation for over forty boys. A dining-room and bathroom have also been built, and the lads will be able to obtain bed and food at a nominal charge. At the rear is a large playground.

Census of Ireland—Leinster and Ulster.

The census returns for Leinster show that there has been an increase of the population in the province of 9,215 during the past ten years. Dublin County showed an increase of 14,826, and county Kildare an increase of 3,061. In Ulster during the same period there was a decrease of 1,130. The only increases in this province were: Belfast, 37,767, and Londonderry, 888; Roman Catholics form 43.7 of the total population.

South Australia.

[FROM OUR SPECIAL CORRESPONDENT.]

Autonomy in Sight.

I finished my last letter (see British Medical Journal, October 14th, 1911) with an intimation that the Australian Branches intended to ask the British Medical Association for such alterations in the rules as might allow of closer federation amongst themselves and of a greater degree of autonomy; and to-day (July 31st) a cablegram has reached us to the effect that our requests have been conceded, and that we shall be granted "a constitution similar to that of the South African Branches." There is a general feeling of rejoicing at the "sweet reasonableness" of the parent Association, and of relief at knowing that our legitimate aspirations have not been opposed. The concession means not only a *federal* strengthening of the bonds of the Association throughout Australia, but also a renewed *imperial* strengthening of those ties which bind medical practitioners here to their fellows in the old country and in the other dominions beyond the seas. Had there been any serious opposition it would have necessarily meant, sooner or later, the dissolution of this imperial tie, for the British-born practitioner is being rapidly replaced by the Australian native, and there would soon arise leaders who "knew not Joseph."

The Federal Council.

And now there should be no delay, for already a provisional Federal Committee has been formed, and it has held its first meeting in Melbourne under the presidency of Dr. W. T. Hayward of Adelaide. It only needs the Association's parental benediction to blossom into a permanent council. But it will want a better name, and I would suggest the Australian Provincial Council of the British Medical Association. It is possible that in time New Zealand may throw in her lot, and then we should style ourselves Australasian, but the difficulty of distance must be considered, for New Zealand is nearly as far from Australia in point of time as New York from Queenstown. It is further suggested that the two weekly papers, the *Australian Medical Journal*, published in Melbourne, and the *Australasian Medical Gazette* of Sydney, should be amalgamated and reappear as the Federal organ and as the actual property of the British Medical Association in all the States.

The Friendly Societies' Dispensary.

In spite of the admirable cohesion of the medical profession in Adelaide and its suburbs, this dispensary was established with a flourish of trumpets; Labour Ministers attended the launching of the ship and spouted the usual platitudes. A staff of three medical men has been obtained from the neighbouring States, all of mature age and all with unrivalled experience; they seemed to be shunned by all but a few of the old "strike-breakers" imported during the hospital trouble of '96.

Annual Meeting.

The annual meeting of the Branch took place at the University on June 27th, when Dr. Hone delivered an admirable valedictory address, dealing mainly with the

subject of infant mortality. He very justly remarked, amongst other things, that the bonus of £5 which the Prime Minister of the Commonwealth proposed to grant indiscriminately to the mother of every infant born during the next year would be a sheer waste, and that the £600,000 involved would be far better spent in attempting to save the lives of the 9,000 infants who annually die within their first year of life. It is some years since we have had an address the equal of this of Dr. Hone's in material affording food for thought. At the dinner in the evening, held at the Café Rubio, the chairman was Dr. Poulton, who is serving a second year of office, having been president of the Branch as long ago as 1893.

Western Australia.

Trade Unionists and the British Medical Association.

An indication of the spirit in which trade unionists in this portion of the globe regard the claims of the medical profession to adequate remuneration for the important services they render is afforded by a recent incident, which cannot be better described than in the following paragraph published in the *Western Australian*, one of the leading daily papers in the district:

"Recently the Yarloop Hospital committee received intimation from their medical officer that in consequence of regulations framed by the Western Australian Branch of the British Medical Association governing the conditions under which members of that Association might contract with benefit societies, hospital districts, etc., for professional services, it would be necessary for his allowances to be increased. The hospital committee found themselves in the position that on the present contribution of 3s. 6d. per month per member, it would be impossible for them to meet the demand and pay other necessary expenses. The committee's report on the subject was the chief business set down for discussion at the half-yearly meeting of subscribers held at Nanga Brook on the 11th inst. That the matter had aroused keen interest was shown by the large number of subscribers, who journeyed from all parts of the concession to be present at the meeting, a special train kindly provided by Millars Co. conveying large numbers from Yarloop and Hoffman. The majority of the subscribers are members of the Timbers Workers' Union, and it was quickly evident that they intended to support their 'union doctor,' as they termed him, one prominent unionist expressing delight at the fact that the medical men were through their 'union' endeavouring to establish a minimum wage. It was decided to bring the doctor's allowances up to the stipulated rate, and authority was given to the committee—should it become necessary—to make a further call of 6d. per month on subscribers. Mr. F. Barraclough, president of the Hospital Committee, occupied the chair."

New South Wales.

[FROM OUR SPECIAL CORRESPONDENT.]

Anthropometric Survey of Australian Children.

During the last thirty years several limited surveys of the physical proportions of Australian children have been made in the various States. The late Dr. F. Norton Manning, of Sydney, recorded the heights of a number of children of the public and private schools of New South Wales, and his results were given to the late Mr. Charles Roberts, and used in his work on *Anthropometry*. But the first important systematic survey was made in Sydney in 1901, the scheme being devised mainly by Dr. Mabel Grabam and Professor Wilson, of the Sydney University. Dr. Mabel Graham, Dr. Mary Booth, and Dr. Brennand carried out most of the actual survey. The results were reported by Mr. Coghlan, Government Statistician of New South Wales, to the conference of the Australian Association for the Advancement of Science, in Hobart in 1902. This investigation, though based on relatively small figures, was a valuable contribution to anthropometric research in Australia, and was the beginning of a systematic attempt to ascertain the characteristics of bodily form exhibited in Australia.

Series of measurements have been made in Western Australia by Dr. Blackburne; in Tasmania by Dr. Elkington; in South Australia by Dr. R. S. Rogers; and in Victoria by the Education Department's medical officers—namely, Dr. Mary Booth, whose previous experience in the work was utilized to establish it, and also by Dr. Jane Greig and Dr. Harvey Sutton. Each year since 1907 the Department of Education of New South Wales has carried out regular anthropometric measurements of the height and weight of school children, and has already records of about 90,000 children, the results being detailed in the department's annual reports. The Commonwealth, in 1907, formulated a scheme and communicated with the States, asking their co-operation in obtaining measurements of school children, with a view to establishing the relations between age, weight, and height, chest measurement, etc. In a paper read at the Science Congress in Sydney, in 1911, Dr. Mary Booth, who has always taken special interest in the work, brought the subject under notice. This led to the appointment by the congress of a committee of experts to encourage anthropometric research and to consider the organization of a systematic survey of school children throughout Australia. The scheme was essentially identical with the former proposals of the Federal Government, but in the interim the report of the British Anthropometric Committee became available, thus making possible a method uniform with that of Great Britain, and so rendering the results immediately comparable with those of Europe.

The Australian Anthropometric Committee has drawn up a memorandum setting forth the object of the survey and suggestions as to the method for the use of teachers, physical trainers, and others interested. The plan of the survey is as follows: The children's heights, weights, and chest measurements are to be taken once a year. A card for each child enables its development to be watched. These cards are to be kept in the schools, and the results tabulated on sheets and sent to the Commonwealth Statistician for the general tabulation which the Federal Government has approved. In several States advantage has been taken by the Anthropometric Committee of the assembling of teachers in physical training camps to arrange for a demonstration on the object and method of the survey. Much interest has been aroused, particularly as regards the physical growth of school children and fatigue effects in connexion with the school programme or drill requirements. It is felt that the question of physique and growth must receive more intelligent attention than in the past if the progress of the British race in Australia is to be properly studied.

Illiteracy in Australia.

The Commonwealth statistician, Mr. G. H. Knibbs, has recently issued an education bulletin, in which he deduces from the number of marks used in the marriage registers instead of signatures the degree of illiteracy in the different States. The bulletin shows that above the age of 9 years there are 63,400 persons who cannot read the English or any other language. Of these 25,600 are in New South Wales and 11,700 in Victoria. Relatively Tasmania is the most illiterate State and Victoria and South Australia the least. In Victoria there are 1,103,428 persons who can read and write English and 3,897 who can read only. Of those who can read and write some other language only there are 4,237, and 598 who can read a foreign language only. There are 173,444 who cannot read at all, but 143,478 of these are under 5 years of age.

Maintenance of Hospitals.

The Hon. F. Flowers, in the course of an address at a meeting of the Sydney University Medical Students' Society, made some interesting statements bearing on the present state of public opinion on the question of what should constitute the basis of our hospital system. He considered it to be the duty of the State to provide for the health of those who were sane, equally with the health of the unfortunates who had lost their mental balance. The whole of the general hospitals of Great Britain were maintained by private generosity, and not by the State. Conditions in Australia, however, were quite different, for

there was no aristocracy, at any rate no hereditary aristocratic classes, and up to the present no very wealthy classes who could afford, or had shown the desire, to provide large endowments for charities. In New South Wales there had been no great bequests or donations for charities, with two or three notable exceptions—the bequest of Mrs. Mary Roberts of between £60,000 and £70,000 divided between the hospitals; the erection and maintenance of the Walker Convalescent Hospital by the late Mr. Thomas Walker and his daughter, Miss Walker; and the erection of the Carrington Convalescent Hospital, chiefly through the benevolence of the late Mr. Paling. Of the millionaires and semi-millionaires who had died during the past ten years not one had given any great sum for the benefit of a special charity or charities. In England there were frequent announcements of bequests of twenty, thirty, and fifty thousand pounds for some hospital, and occasionally even a quarter of a million or half a million for the same purpose. In Australia, a man with an income of £10,000 to £20,000 a year gave £10 to perhaps two or three hospitals, and that was regarded by him as his limit. Perhaps when he died he might leave £100, or even £1,000, to some institution. He expected, if all the endowments of all the hospitals in New South Wales were totalled up they would not amount to anything like a quarter of a million pounds, representing, say, £10,000 a year in the way of revenue.

Since this address the gift of £1,000,000 to the charities of Australia from Mrs. Walter R. Hall has been announced. The late Mr. Walter R. Hall, who died in Sydney in October, 1911, left his widow the residuary legatee of his estate (after deducting provision for those whom he desired to benefit at his death), and (1) to the charities of New South Wales the sum of £20,000, and (2) to the charities of Queensland the sum of £30,000. His widow has decided to establish a monument to her late husband's memory, and has handed over to trustees securities to the value of £1,000,000, the income from which is to be for ever devoted to the purpose of charity. Charity, by the deed which carries into effect her wishes, is defined to mean: (1) The relief of poverty; (2) the advancement of religion, according to the tenets of the Church of England; (3) the advancement of education; (4) the general benefit of the community not falling under any of the preceding heads. The benefits from the application of the annual income of the trust are to be allocated as follows—one-half in New South Wales, one-quarter to the State of Queensland, and one-quarter to the State of Victoria; one-third of the income to which each State is entitled is to be distributed primarily for the benefit of women and children. The trust is to come into operation in January, 1913.

Naval Surgeons.

A conference was recently held in Melbourne of representatives from the Universities of Sydney, Melbourne, and Adelaide, with the Naval Board, to consider the question of the supply of surgeons for the Royal Australian Navy. The following scheme was drawn up by this conference: Candidates must be of pure European descent and of Australian birth, or permanently domiciled in Australia. They must be nominated by the senate or council of an Australian university, on the recommendation of its faculty of medicine; they must have graduated in medicine and surgery at an Australian university, and have had at least one year's experience in a general adult hospital recognized by the university. Their age must not exceed 28 years, except in special circumstances, when the age limit may be extended to 30 years. Nominations will be allotted among the universities in order of seniority, the present requirements being as follows: Sydney, 2; Melbourne, 2; Adelaide, 1. Subsequent nominations, which will be numerous, will be allotted in rotation to Sydney, Melbourne, and Adelaide. The Defence Department is now inviting applications for the positions of staff surgeons, in accordance with the above regulations. Successful candidates will be required to engage for three years, and will have the option of engaging for a further period of three years. The successful applicant will be sent to England as soon after appointment as practicable to undergo a short course in a Royal Navy hospital, or in one of His Majesty's ships, prior to joining H.M.A.S. *Australia*. The rates of pay are, on entry, £547 10s.; over two years, £593 2s. 6d.; and over four years, £638 15s. These rates include active and deferred pay.

India.

[*FROM OUR SPECIAL CORRESPONDENT.*]

Kala-azar in Assam.

A supplement to the sanitary report of the Province of Eastern Bengal and Assam for the year 1910, recently published, contains some special reports submitted by medical officers to the Government during the two preceding years. The subject of the first of these is an inquiry regarding an outbreak of kala-azar in the Golaghat subdivision of the Sibsagar district by Major S. R. Christophers, I.M.S. The disease had died out after severe prevalence in the adjoining district of Newgong six years previously. Golaghat is on the way to the districts of Sibsagar and Lakimpur in Upper Assam, and the important questions referred to Major Christophers were: (1) Whether kala-azar really prevailed in Golaghat; (2) if so, whether it was on its spread northwards; and (3) how its march in that direction might be prevented. To the first question an affirmative answer is given, founded on clinical evidence, including splenic puncture. As regards the second, it is stated that advance seems to be very slow, and that the disease has died out in some villages. With respect to prevention, isolation of the sick and contacts, and precautions against migration appear to be the most promising measures.

Treatment of Leprosy.

Colonel Neil Campbell, I.M.S., contributes to the same supplement an interesting note on the treatment of 16 cases of leprosy by hypodermic injections of nastin B. Eleven of them were inmates of the Leper Asylum, Sylhet, one a prisoner in the Rampur-Boalia Gaol, and four out-patients of the Ganhati Civil Hospital; 1 c.cm. of the drug was injected with strict aseptic precautions into the subcutaneous fat every fifth or seventh day. The treatment is tedious and expensive, lasting over months and requiring patience and careful watching. Local reactions were observed in some of the cases. The results obtained in the Leper Asylum by Drs. McCoy and Ashe were satisfactory. "In no case have fresh lesions appeared. In every case there has been more or less improvement." Benefit resulted as regards both trophic and nervous lesions, and the general health of the patients underwent improvement. The other cases treated at Rampur-Boalia and Ganhati were failures, the patients getting tired of treatment and ceasing to attend.

Epidemic Dropsy.

As in the epidemic of 1877–8–9–80, so in the case of the outbreak of 1907–8–9, the prevalence of epidemic dropsy in Calcutta has been followed by the appearance of the disease in other parts of Bengal. Two reports in this supplement describe two well-marked invasions of townships of Eastern Bengal—one in the Habigmij subdivision of the district of Sylhet, and the other in the town of Faridpur and in neighbouring villages. The former outbreak was investigated by Assistant Surgeon H. Lungdoh and the latter by Captain T. C. McCombie Young, I.M.S. In Habigmij the disease broke out in December, 1909, and attacked 50 persons, of whom 2 died. The cases occurred in groups, and whole households suffered, but not simultaneously. The dropsy was in most cases preceded by diarrhoea, and "slight fever was present in all cases." No marked symptoms of peripheral neuritis were observed. The reporter considers that the malady was epidemic dropsy, and not beri-beri, failed to find any dietetic cause, and is inclined to consider the disease as a specific infectious one. In Faridpur the first cases occurred in December, 1909, and in February Captain Young found 48 cases scattered through the town and adjoining villages. The symptoms resembled those of the Habigmij outbreak, and both Captain Young and Dr. Ashe, the Civil surgeon, arrived at conclusions similar to those above noted, considering the disorder an infectious one, not caused but perhaps conveyed by food. Some evidence of importation from Calcutta was obtained.

Major E. D. W. Greig, I.M.S., has submitted a second and final report regarding the outbreak of epidemic dropsy in Calcutta which he was deputed to investigate. This report does not advance the subject beyond its predecessor, and is equally open to the strictures passed on the first (vide BRITISH MEDICAL JOURNAL, vol. ii, 1911, p. 391). It is quite inconclusive as regards identification, causation, and prevention. The deductions arrived at depend largely on oral and statistical evidence of very doubtful value, and there is a conspicuous absence of control observations. The identity of epidemic dropsy and beri-beri is taken for granted, and the whole contention is virtually a *petitio principii*. The experiments on fowls, both original and quoted, demonstrate that degeneration of nerves (so-called neuritis), resulting in paralysis, can be induced by special dieting involving phosphorus starvation, and cured by restoring the deficient elements; but, curiously, in not a single experiment was dropsy, the invariable and essential feature of this disease, observed. The observations on dieting possess some collateral importance. They are of interest as regards true beri-beri—endemic polyneuritis—and on general grounds, but their significance as respects epidemic dropsy is more than questionable.

THE PUNJAB LUNATIC ASYLUM.

The Legislative Council of the Punjab has passed a complimentary resolution on the report of the Punjab Lunatic Asylum at Lahore submitted by Lieutenant-Colonel Ewens, I.M.S. The Lieutenant-Governor states that the institution owes a deep debt of gratitude to the Franciscan sisters who are in charge of the nursing arrangements of female patients. During the past three years there has been a steady increase in the comforts and amenities of the asylum, and the new buildings, providing accommodation for 186 patients, have greatly improved the conditions of life. It is hoped that further improvements will be possible when definite schemes have been prepared. The poorer classes supply the bulk of the patients, and 70 per cent. of the admissions are persons between the ages of 20 and 40—doubtless the period of life at which lunatics can be least effectually looked after by their friends.

CHOLERA IN SIND.

Four of the six districts in Sind are now more or less affected by cholera, besides the native State of Khairpur. The epidemic seems to be worst in the Larkana district. It is the villages that are mainly affected, the towns being practically free. Precautionary measures are being adopted everywhere.

THE SIMLA MUNICIPALITY.

The annual report of the Simla Municipality states that no decision has as yet been reached in respect of the extension of the municipal boundaries. In the course of the year 240 building applications were received, each of which called for an inspection of the site and a detailed scrutiny of the plans by the several officers concerned before submission to the committee. To avoid accentuating the evils of overcrowding the committee were obliged to refuse a certain number of applications or return them for considerable modification. On the other hand, not only have they sanctioned buildings in open areas, but have provided and improved drains, roads, and lighting, as, for instance, at Dhar, a suburb with a rural charm of its own which appears to offer some compensation for its remoteness from the business centre of Simla. Buchail also is a locality in which there is some indication of a desire to build houses for the Indian community, and here also municipal amenities have been or are being provided. The committee recognize that if they are, under provisions of the new Act, to restrict buildings in congested areas, they must offer facilities in open spaces to furnish the increasing population with suitable residences. But the task of making roads and of providing water and sewage pipes for single houses, even with the promise of an increase in their number, is one that has to be approached with extreme caution. The summer population of Simla, after rising quickly in the thirty years between 1869 and 1898, has been practically steady during the last seven years. Thus the summer census of 1869 showed 1,434 Europeans and over 13,000 Indians, giving a total of 15,018. That of 1889 showed 3,400 Europeans and 20,779 Indians, or a total of 24,179. That of 1904 showed 4,096 Europeans and 31,608 Indians, a total of 35,704; and that of 1911 showed 4,153 Europeans and 31,849 Indians, giving a total of 36,002.

Special Correspondence.

PARIS.

Congress at Nimes.—Maternity Benefits for Paris.

AT the recent congress at Nimes, Dr. Bergonié, in discussing the treatment of stricture of the urethra by electrolysis, stated that the tightest stricture yielded without bleeding, and the patient was able to continue his ordinary routine of life during the treatment. Dr. Roques, of Bordeaux, spoke favourably of the results of radio-therapy in chronic tuberculous adenitis. There was appreciable diminution in the size of the swellings, lessening of the periadenitic thickening, and diminution of suppuration. Eventually the indolent keloid edges of the wound became more healthy, and a satisfactory cicatrix was obtained. In intrathoracic adenitis the *x* rays produced amelioration in the symptoms, and notably lessening of chronic cough and gain in weight. In abdominal glandular swellings likewise good results had been obtained. Dr. Cerira Salse (Barcelona) gave an interesting account of results obtained with high-frequency treatment in tuberculous disease of the bone. He cited three cases—the first that of a young man who had necrosis of the humerus following an injury; after two months' treatment not only was the bone solid and well calcified, but the function of the arm was normal. Like results were obtained in another similar case, and also in a case of tuberculous dactylitis. Dr. Miramond de Laroquette described the favourable effect of light-baths in various diseases of the skin and bones. He stated also that certain rays had powerful action in fractures where good union had not occurred owing to rarefaction of the osseous tissue. Good union and healing had been noticed in cases of fracture of the tibia in the middle and lower thirds. The treatment of cancer was discussed, and Dr. Julien reported favourably on the action of radium in cancerous growths of the cervical gland, and in cancer of the breast; in one case, in which the radium tube was placed in the centre of the tumour, the swelling disappeared with extraordinary rapidity. Dr. de Keating Hart spoke on the treatment of cancer by fulguration, and reported cases cured after periods varying from five to three and a half years.

The Paris Municipal Council has arranged a scheme for the assistance of expectant mothers who have lived two years in Paris. The women will be required to give notice before the seventh month of pregnancy. The husbands must not be earning more than 1,800 francs (£32) per annum. The scheme proposes that the municipality shall give 20 francs to the Mutualité Maternité towards the care of the mother and child. The scheme is to come into force on January 1st, 1913.

VIENNA.

Post-graduate Courses at the General Hospital.—Professorial Appointments.—Fresh Outbreak of Cholera and Small-pox.—Discontinuance of the late Professor von Neusser's Clinic.

A SERIES of post-graduate classes has been arranged at the Vienna General Hospital during the months of July, August, and September for the benefit of busy doctors who can only find time during the summer holidays to keep themselves abreast of the latest medical and scientific discoveries. These classes are even more popular with foreign graduates than with Austrians, so that of recent years it has been found necessary to divide the courses into German and English, applicants for the latter being extremely numerous. Moreover, the number of students in each class has been restricted to ten, and the fee is very low—from £1 15s. to £2 10s. a month. The classes are usually given either by the clinical assistants or by lecturers (*Privatdocenten*) who have formerly held posts as assistants at the various clinics and are still attached to one or other of them. The list of classes is published in the leading German medical

papers some weeks before the commencement of the session; and measures are taken to render this list easily accessible to English and French readers. On the other hand, doctors coming from Greece, Turkey, or the Balkan States have to find out for themselves particulars regarding such classes as are open to them.

A large number of new professors have been appointed at the University of Vienna during the last twelve months, the candidates being nominated by the Emperor, or rather by the Ministry, which controls both schools and university. The list includes the names of several eminent men, such as Hajek, Hammerschlag, Alt, Ewald, Hamburger, and Bartel, who are well known in the English medical colony in Vienna. The proportion of doctors with academic titles (professors and *Privatdocenten*) amounts to more than 10 per cent. of the total number of doctors of medicine practising in Vienna, and this has given rise to a certain amount of ill feeling on the part of the general practitioners, who complain that the holders of these titles, in the majority of cases, do not content themselves with being merely consultants. Most of them are specialists, and treat their patients as though they were regular practitioners; and the difference between the fees of an ordinary doctor and those of a titled specialist is so small that the public prefers to go directly to the specialist, who, by the way, considers himself well paid by a fee of 10 kronen (8s. 4d.). It is hardly surprising, therefore, that the bulk of the profession wish to see some restrictions made on the practice of appointing professors whose sole duty seems to consist in putting a title before their names.

The wet summer with which, in common with the rest of Europe, Austria-Hungary has recently been afflicted, has had one good result, inasmuch as the rain has kept the air free from the clouds of dust which are so often a prolific source of danger during the hot months. It has also, in consequence of the decrease of summer diarrhoea, prevented the usual heavy mortality amongst children; and flies were far less numerous than is usually the case at this season. On the other hand, cholera has recently appeared in the Austro-Hungarian border districts, though so far the Board of Health has succeeded in keeping it well under control; and sporadic cases of small-pox have been notified from all over the country, one case having occurred in Vienna itself. Every means have been taken, however, to stamp out infection, so that at present the empire is in no danger of being ravaged by these much-dreaded diseases.

The death of Professor von Neusser has been the cause of considerable anxiety to the medical students at the University of Vienna, since there is a rumour that his clinic is about to disappear. The wards will probably be divided amongst the three other medical clinics (von Noorden, Chvostek, Ortner), though the ever-increasing number of medical students would justify the introduction of even another (and fifth) clinic. The decision of the Government—which, as usual, depends entirely upon questions of finance—is therefore awaited with impatience in medical circles.

BUDAPEST.

The Defects of Medical Terminology.

THE name of Dr. V. Austerlitz, Assistant Professor of Anatomy at the Kolozsvár (Klausenburg) University, is to be added to the long list of critics of medical terminology. Its defects, he maintains, lead to many misunderstandings and difficulties. For instance, it is most objectionable that the name of some symptoms or disease should convey an idea remote from the condition it ought to designate. The reason of this is to be sought in the fact that such denominations are of ancient origin, dating back, in some cases, to mediaeval times when clinicians swore by the humoral pathology, and anatomists had to restrict their activity to the dissection of animals only. The upper rectus muscle of the eyeball used to be called the *Musculus religiosus*, because it moved the eye toward the sky, and the lower rectus *Musculus humilis*. For a similar reason retention cysts of the cervical mucosa of the uterus are called "ova" (*Ovula nabothi*), and sadness "melancholy" (black gall), and muscle pains *rheuma* (flow).

Another source of unsatisfactory terms is the fact that the names of those who have first described certain symptoms have been used to denote the condition. Thus in the history of middle-ear catarrh such oddities may be read: "Weber positive, Rinne negative, Schwabach protracted." In some instances priority is claimed for three or four persons, and in this way such terms are created as "Charcot and Leyden crystals," "Cheyne-Stokes respiration," "Roser-Nélaton line," "Wassermann-Neisser-Bruck reaction." The absurdities arising from the use of authors' names is illustrated by the so-called "Patella disease," a term sometimes applied, not to any disease of the patella, but to the stenosis of the pylorus observed in certain tuberculous patients. The symptom derives its name from Vincenzo Patella, an Italian professor in Siena.

Another feature of medical terminology to which Austerlitz takes exception is that some affixes—for instance, the "itis" affix for inflammation—are not used in a consistent manner. As a rule, they are affixed to Greek terms, but in some cases to Latin. For instance, metritis is used instead of uteritis and colpitis instead of vaginitis, but in tendovaginitis the "itis" is affixed to a Latin root, and similarly in bursitis, gingivitis for tonsillitis. Sometimes the "*itis*" is affixed to an author's name, giving rise to absurd and ridiculous names — for example, Bartholinitis, which word, properly interpreted, would mean the inflammation of Bartholin, the anatomist. There are inflammations which are not expressed by "itis"—for example, pneumonia, phlegmone. At the same time, "*itis*" is affixed to diseases which are not inflammatory—for example, rachitis, diphtheritis, though the last-word has now fortunately been generally replaced by diphtheria.

For the designation of tumours it is customary to affix "oma"—for example, sarcoma, chondroma, carcinoma—but glaucoma and trachoma have nothing in common with tumours. Habit has made such terms, and many more might be quoted, so familiar to medical men that their incongruities pass unnoticed, but they are bewildering to medical students and to educated members of the public who seek etymological explanations. Dr. Austerlitz suggests that the International Medical Congress to be held in London next year (1913) should take the subject of medical terminology seriously into consideration.

Correspondence.

PERIBRONCHIAL PHTHISIS.

SIR,—Dr. A. C. Jordan raises an interesting question in his paper upon peribronchial phthisis. He does not, however, seem clearly to realize the differences in the method of infection in children and in adults. In children morbid anatomists have long recognized that infection takes place in most instances through the root of the lung.

The only illustration given by Dr. Jordan in which a *post-mortem* examination was made occurred in a child, and conclusions of far-reaching character can scarcely be based upon such a case. That infection in adults occurs almost invariably at a certain spot at the apex cannot be doubted. This spot I believe to be the quiescent area indicated many years ago by my fellow-student, Dr. H. J. Campbell. The absence of infection in children at this spot is probably due to the movements and shape of the chest. Within the chest of the child there is probably more complete aëration of the whole lung than in the adult. In the fully developed chest the area of air stasis occurs at the apex of the lung, and there the bacilli lodge. While the method of infection in children is generally through the root of the lungs, it is not clear to me that the case recorded by Dr. Jordan is of this type. To me the description reads more like infection through the apex. However that may be, infection through the root of the lung in children virtually always occurs through the medium of caseous bronchial lymphatic glands, and in this case the glands were not caseous. In children, it may be mentioned, the lower lobe is almost as likely to suffer as the upper—that is to say, infection not very uncommonly invades mainly the lower lobe, leaving the upper lobe free.

These last remarks apply chiefly to acute and subacute infection. My experience leads me to believe that chronic tuberculous disease of the lungs in children is very uncommon. Caseous bronchial glands unfortunately are far

from rare, but when infection arises through them the case almost invariably progresses rapidly to a fatal ending. Instances of chronic disease of the lung in children in the great majority of cases prove to be non-tuberculous.—I am, etc.,

September 2nd. THEODORE FISHER.

TUBERCULIN TREATMENT.

SIR,—I was somewhat troubled and not a little perplexed on reading Dr. Camac Wilkinson's letter in your issue of September 14th. I am afraid he has somewhat misunderstood my intentions. When I wrote my original letter I was desirous, owing to my ignorance, to find out, if possible, why there was so much discrepancy of opinion as to the advisability or otherwise of administering tuberculin, and so great divergence of opinion as to the results obtained and the method to be adopted if the treatment is to be successful.

The recent Hunterian Oration would seem to show that some physicians who should have had some experience of this disease are not convinced of its value. Dr. Thomas Glover Lyon concluded his oration by saying, among other things, that no reliance can be placed on specific remedies. When such conflicting views are held, it seems to me that it is a matter which should not be allowed to rest. Dr. Wilkinson must be as interested as any one else in having this matter settled authoritatively, and it is obviously of paramount importance to the patients concerned that as few mistakes should be made as possible. From my own somewhat limited experience, I am convinced that Dr. Wilkinson is far more in the right than Dr. Glover Lyon; yet I do not think it can be said that finality has been reached in this matter, and even Dr. Wilkinson may still be able to learn a little from others.

I was glad to learn that Dr. Wilkinson is in favour of commencing treatment with relatively small doses. My reason for asking this question was that I had gathered from medical men who had been in close contact with Dr. Wilkinson that he was not quite sure in his own mind whether it might not be more dangerous to use small doses than larger ones, a proposition which might be quite possible of defence when we consider the intricate and complicated workings of such a remedy as tuberculin. I did not wish it to be inferred that all my cases did badly on account of the methods which I adopted.

With regard to the two cases mentioned by Dr. Wilkinson, I have no doubt if all the data were forthcoming they might be classified. Curiously enough, two days ago I had a letter from an old patient of mine who, a little more than four years ago, was admitted into a santorium of which I was then in charge. He had previously been for eight or nine months resident in a sanatorium, and after that he had spent some months at a health resort, at the end of which time he was a great deal worse than when he had first gone to the sanatorium and had very extensive disease, tubercle bacilli in the sputum, and appeared quite hopeless about himself. At the end of four months' treatment, without tuberculin, he was completely restored to health, and has been well ever since, and is at present medical officer to a tuberculosis dispensary in one of our large cities. I am inclined to think that he might have benefited with tuberculin, but the fact remains that he did well without it. With regard to such cases, I may perhaps be permitted to point out that when a patient goes on for a long period gradually getting worse, so that extensive physical signs are present, and, it may be, abundant tubercle bacilli, it may be implied that the tissues of the patient are very resistant, or that the bacilli concerned are of less than average virulence, otherwise a fatal termination would be more speedily reached. Such cases, as a rule, are the result of neglect of efficient treatment; some of these cases are, I am inclined to think, incurable, unless tuberculin be administered, and may present excellent examples of the triumphs of tuberculin. I should be quite inclined to the belief that Dr. Wilkinson has saved the lives of both these patients.

I may mention that an astute sanatorium physician will often accept cases such as the above, knowing full well they will be easier to treat, and produce more permanent and brilliant results than many so-called early cases, which, because early, are often thought by the unwary to be more favourable.

There is one aspect of the question which I should like to emphasize. It is not so much necessary to know what happened to a number of individuals who reached big doses of tuberculin, or who were able to pass through all the grades in a system of graduated labour, as to know how far the system of treatment has been successful in bringing the cases to such a favourable condition.—I am, etc.,

Ayrshire Sanatorium, Sept. 16th. EDWARD E. PREST.

TUBERCULOSIS COLONIES.

SIR,—I was glad to see the letter by Dr. Benjamin Moore, in the JOURNAL of August 24th, insisting on the importance of preventive as contrasted with curative measures for dealing with tuberculosis. I fully endorse his view that sanatorium and dispensary treatment of curable cases is but a lopping of branches which will be to a large extent futile, from the preventive point of view, unless at the same time a vigorous attack is made on the root of the trouble—the sources of infection.

There appears to be ample evidence for the conclusion that at present "infection is so widespread that the artisan class—attending as children public schools, and as adults working in factories and travelling in public conveyances—can hardly hope to escape it, and, as is shown by *post-mortem* data, do not escape it,"[1] although the bulk of the population have sufficient resistive power to survive the attack, and die from other causes, only about 8 per cent. of the total death-rate being due to tuberculosis.

It seems to me that the two preventive measures of cardinal importance are:

1. Ensuring a pure milk supply, free from tuberculous infection. (The State ought also, of course, to institute a thorough-going campaign against tuberculosis in domestic animals.)
2. The segregation of patients who expectorate tubercle bacilli.

Both will be difficult to enforce, and compulsory segregation, if attempted at the present time, would doubtless meet with bitter and determined opposition. But in these days public opinion is being transformed with startling rapidity by the leaven of scientific discovery, and the old views regarding the liberty of the subject are showing manifest signs of disintegration. If liberty, in the case of an individual, means the risk of passing on to others an insidious, lingering, and often incurable disease, we are justified in curtailing that liberty to the extent that is necessary for the safety of others. The principle has for thousands of years been admitted in the case of leprosy, which, although more disfiguring, is probably less infectious than tuberculosis, and in recent years public opinion has sanctioned its extension to one disease after another. We have only to produce and establish clear proof of the necessity for its extension to tuberculosis for public opinion to sanction that too.

Many, no doubt, imagine that if patients are careful about the disposal of their sputum, the risk to their neighbours is negligible—that, for instance, a patient who has been trained in a sanatorium and is conscientious in carrying out the precautions he has learnt, may safely return to his family, even although he continues to expectorate millions of virulent bacilli. Any one who is in the habit of closely observing tuberculous patients must see, however, that although danger may be greatly reduced in this way, it cannot, even with the best intentions, be entirely obviated, and that complete safety can only be obtained by segregation of the infectious. After all, the necessary curtailment of the patient's liberty is not such a dreadful thing as many probably imagine. Segregation need not entail prison walls and an end to all joy and usefulness in life for the unfortunate victims; and for curable cases it will only be temporary. For well-to-do patients, private sanatoriums as at present provided are quite suitable, but these institutions ought to be put under the supervision of the proposed State Board of Health, whose requirements they would then have to fulfil. The

[1] Karl Pearson, F.R.S., *The Fight against Tuberculosis and the Death-rate from Phthisis*. London: Dulau and Co., Limited, 1911.

difficult problem is to find suitable accommodation for the enormously greater number of patients belonging to the poorer classes. For the very advanced cases, which are also the most dangerous, hospital or sanatorium accommodation, where the patients can be nursed and made as comfortable as circumstances permit, is the only provision required; but for earlier cases, still fit for a certain amount of work and play, I suggest something different from the ordinary sanatorium. The idea is not a new one, and has already been carried out on a small scale with marked success.

My proposal is that a number of large colonies be established on suitable sites throughout the country, where consumptives could carry on a great variety of occupations—farming, gardening, building, carpentry, designing, clerical work, storekeeping, domestic service, factory work, and all the multifarious activities of a large community. It has been abundantly shown by Dr. Paterson (at Frimley) and others that while overwork is fatal to consumptives, an amount of work that is within their powers is beneficial, and far better for them than idleness. The work must, however, be suited to the patient. To put an elderly clerk of frail physique to manual labour on a farm might be disastrous. Hence the necessity, if the colony is to be successful, of providing a sufficient variety of occupations.

The whole life of the colony would, of course, be under the supervision of expert medical officers. Patients would work eight, six, or any smaller number of hours as prescribed. Many a man with early lung trouble, who is not quite able for a full day's work, has at present no escape from the cruel alternatives of putting in full time and struggling on with work that is killing him, or else giving up his job altogether. In the colony these men would be enabled to continue such work as they were fit for, with benefit both to themselves and the community. Patients would receive remuneration for their work, partly in the form of board, lodgings, and treatment, and partly, if funds permitted, in cash. They would live under sanatorium conditions and be given the best possible chance of recovery or improvement. Ample facilities for recreation and entertainment would be provided, and open-air (or fresh-air) religious services for those who wished to take part in them. All the work of the community, except that of the most responsible administrative posts, would be carried on by the patients themselves and the colony would be to a large extent self-supporting. The State or public authority ought, of course, to be responsible for the finance and administration, but a noble field would be left for private beneficence which could help in countless ways to improve the amenities of the place and make life pleasant for the patients. Experience as a sanatorium superintendent has convinced me that life in such a colony could be made quite cheerful and attractive, entailing little hardship except separation from friends which need not be absolute.

As many public bodies are at present contemplating extensive expenditure for the treatment of consumptives, I hope that some of them may be induced to start an experimental colony on these lines. Admission might be voluntary, and I believe that a great many patients would gladly avail themselves of such an opportunity of working under conditions which would give them the best chance of cure. Later on, if the experimental colony were successful, the system could be extended so as to provide for the whole of the (more or less) able-bodied consumptive population. By such a scheme the most fatal objection to sanatoriums, their expense, would be reduced to a minimum, while their advantages would be fully retained.

Given a pure milk supply and effective segregation of infectious patients, I believe that the incidence of tuberculosis among the general population could be abolished, almost at a stroke. Of course, the colonies would have to go on for many years, but once the system was in full working order, the number of admissions would very rapidly drop and the colonies could gradually be reclaimed for the use of the general public.

Without such drastic preventive measures, I fear we shall not get rid of the scourge of tuberculosis until in the course of generations Nature achieves the immunity of the race by the slow but steady elimination of the susceptible—that ghastly, heartrending process with which many of us are so terribly familiar.—I am, etc.,

J. E. ESSLEMONT, M.B.

West Southbourne, Hants, Sept. 15th.

DEVELOPMENT OF THE NOSE.

SIR,—While looking through some accumulated numbers of the JOURNAL I have just come across a notice—in the issue of August 17th—drawing attention to some recent papers of mine on the development of the nose, throat, etc.; in this you refer, among other things, to certain observations made by me on the earlier development of the pituitary body.

When, in 1910, I described the remains of Rathke's pouch as ultimately extending along the edge of the nasal septum as far as its angle of junction with the palate, I did not know of any similar previous observations. Professor Tourneux and his son, however, had already described the remains of the pouch in this situation in a communication to the Association of Anatomists at Lille in 1907, a fact of which I only became aware through the courtesy of Dr. Tourneux in sending me a copy of his paper. Under these circumstances it is fitting that I should direct the attention of those interested to this paper and to a couple of later associated papers.

The references are: "Developpement de la base du crâne et de la paroi postérieure du pharynx," Assoc. des Anatomistes, Congrès de Lille, 1907; "Base cartilagineuse du crâne et organes annexes," published at Toulouse, 1911; "Pédicule hypophysaire et hypophyse pharyngée chez l'homme et chez le chien, canal cranio-pharyngien et canaux basilaires," *Journ. de l'anatomie et de la phys.*, No. 3, 1912.

I think these writings may be of some interest and importance to any of your readers who may be working at the pathology of the pituitary, vault of the pharynx, or base of skull, and for this reason I venture to ask for space for the correction in your JOURNAL.—I am, etc.,

J. ERNEST FRAZER.

St. Mary's Hospital Medical School, Sept. 10th.

INTRAPERITONEAL INJECTIONS OF OXYGEN.

SIR,—Mr. H. J. Godwin's note on "Intraperitoneal Injections of Oxygen during Certain Abdominal Operations" (September 14th, p. 630) raises several interesting points. The intraperitoneal administration of oxygen has been in use for some time. Schulze (*Mitt. aus. d. Grenz. d. Med. u. Chir.*, Bd. 18, p. 150) in 1908 recorded seven cases of tuberculous peritonitis treated successfully by evacuation of the fluid through a cannula and the subsequent distension of the abdomen with oxygen.

As a post-operative measure in cases of septic peritonitis the method described by Weiss and Sencert (*Rev. de Chirurgie*, April and May, 1910), of passing a continuous current of oxygen through the tubes draining the peritoneal cavity, would appear to be preferable to that of distending the abdomen with the gas; for, if the peritoneal infection be not generalized at the time of operation, any separation of peritoneal surfaces is to be avoided, whilst, if it be generalized and the patient very ill, the question of respiratory embarrassment immediately comes in. For the past two years this method of passing a continuous current of warmed oxygen gas through the pelvic drain in cases of severe peritonitis has been practised in several of the surgical clinics in Edinburgh, and apparently with benefit.

As Seaman Bainbridge was the first to show, oxygen is rapidly absorbed from the peritoneal cavity; it is, however, an irritant, as evidenced by the active hyperaemia of the peritoneum and the free transudation of serum and leucocytes which it produces, hence probably its value in tuberculous peritonitis.

In an animal under an anaesthetic moderate distension of the abdomen with oxygen has, in my experience, little or no beneficial effect on the general circulation; in some cases, indeed, it caused an acceleration of the pulse and a fall in blood pressure. The propriety of filling the peritoneal cavity with oxygen after operations for the removal of large abdominal tumours or for adhesions is doubtful. In the light of Yandell Henderson's work on acapnia it would appear that the sluggishness of intestinal peristalsis which tends to follow abdominal operations is due to the

aëration of the viscera from exposure during the operation. As he has proved experimentally, this paresis may be avoided by exposing the viscera to an atmosphere of carbonic acid gas. I would suggest that after operations for adhesions, carbonic acid gas instead of oxygen should be injected into the abdomen by the excellent method described by Mr. Godwin.—I am, etc.,

Edinburgh, Sept. 16th. D. P. D. WILKIE.

PERFORATION OF THE TYMPANUM AFTER SYRINGING.

SIR,—Under the title of "Perforation of the Tympanum after Syringing," Dr. Drybrough-Smith called attention in the BRITISH MEDICAL JOURNAL of July 27th, p. 181, to the sequela of removing wax from the ear. The value of wax is somewhat noticeable in Natal and the Cape Provinces, where the pain and head-noises associated with a dry condition of the drums are aggravated by dry winds, particularly the hot ones, which are so frequent in this climate at certain periods of the year. The natives do not care to venture out on a windy, dusty day, the coolies wrap their ears up in flannel, and the Europeans often stuff their ears with cotton-wool. All this seems to indicate the need for protecting the drum from the wind, and a large piece of accumulated wax in the ear may act as a useful protection against the dry winds. Remove the wax and this protection is gone, with the result that the drum becomes dry, and a tendency to otorrhoea is increased. This is a much more likely cause than that of forcible syringing, if the nozzle of the syringe has not been directed on to the drum of the ear. However, considering the risks of exposing a drum which has been protected for years by a piece of wax, it is always desirable to use a small plug of cotton-wool to protect the drum for a few hours after the wax has been removed, and to advise the patient to use a drop or two of warm oil to keep the meatus moist, provided, of course, that there is no discharge from the ear.

Can you suggest a really satisfactory treatment for noises in the ear besides the obvious one of looking after the condition of the meatus, nasal passages, and Eustachian canal? General tonics are, of course, useful, and potassium iodide influences some cases, but adrenalin does not seem to produce much effect; and Mr. Heath's treatment of painting the drum, which is recommended by some otologists, seems rather a drastic measure when there is not much deafness associated with the condition. —I am, etc.,

F. G. CAWSTON, M.B., B.C.Cantab.

Greytown, Natal, Aug. 17th.

PROPOSED COMMISSION ON DIET AND DISEASE.

SIR,—In Professor Leonard Hill's address to the British Association occurs the striking observation that out of 2,000 children at Dr. Barnardo's Homes not one had had appendicitis. This statement requires examination, but if true it suggests that appendicitis is due to too liberal a diet, for probably it is in more stinted diet that the manner of life of these children chiefly differs from that of children in more affluent circumstances. The question of the connexion of diet with health and disease has received much attention of late years, but we still seem to be at sea on the subject. On one hand, we have Dr. Haig, the Hon. Rollo Russell, and many other authorities, both medical and lay, attributing numerous diseases to excess of certain articles of diet, and that general moderation promotes health and longevity is a truism. On the other hand, it is clear to those who have eyes to see that the diet of all but the very poorest classes greatly exceeds what is necessary for sustaining life, and we have medical journals advocating a more "liberal" diet in public schools, and the general practitioner putting down many diseases to "malnutrition" and seldom suggesting that the "mal" is due rather to excess of than to too little food.

Personally I am inclined to believe that for preventive and curative purposes general limitation of food is more important than deprivation of certain classes of nutriment. How, otherwise, explain the boasted success in rheumatism of the "Salusbury treatment" (minced meat only) on the one hand and the uric acid free diet on the other?

The Chittenden experiments and the fasting experience of various individuals surely demand further investigation. Might not the Royal College of Physicians be asked to appoint a commission to investigate and let us know whether we are moderate or whether we are most of us, gourmands who are being plagued for our gluttony by cancer, diabetes, and appendicitis, in addition to gout and the other diseases long recognized as the outcome of excess?—I am, etc.,

Woolwich, Sept. 16th. SIDNEY DAVIES.

MEDICAL SCIENCE AND WEIGHTS AND MEASURES.

SIR,—I do hope Dr. Johnston-Lavis's witty protest in the JOURNAL of August 31st about the British standard weights and measures will open the eyes of the profession in England.

Having often to examine candidates for an English life insurance company, I have to ask a Continental candidate how many pounds and stones his weight is and how many feet and inches his length!

Really I cannot believe that they do not know the existence of something like a Kgr. or a M. at the insurance office, but not to use the metric system in Continental insurance work goes above my understanding.

I feel sure that the imperial measures strongly impede the propagation of English medical literature in foreign countries.—I am, etc.,

Haarlem, Holland, Sept. 7th. H. BOSHOUWERS.

SIR,—A member of the Clinical Research Association has called our attention to the letter of August 31st by Dr. H. F. Johnston-Lavis, to the effect that, on his complaining about the results of a certain analysis being presented as grs. per gallon, the Secretary of the Association wrote him "a plaintive letter saying, of course all their researches were made by metric measures, but the ignorance of English medical men was such that they had to convert them to standard measures."

The member in question asks whether it is the fact that we wrote in such an "insulting" manner of English medical practitioners, and perhaps the best reply to this inquiry is to quote the actual terms of the Association's letter to Dr. Johnston-Lavis, in which we say that:

> When we cannot get a small number of educated Englishmen, as the members of the medical profession are, to change from antiquated apothecaries' tables to those which have for their basis, ease, simplicity, and generality, we cannot hope for such a radical change from the populus.

It appears to us that this is a plain statement of facts to which it is difficult to take exception, and your readers may judge for themselves whether the tenor of our remarks is accurately reproduced by your correspondent. —I am, etc.,

CHAS H. WELLS,

London, Sept. 10th. Secretary, Clinical Research Association.

THE INDIAN MEDICAL SERVICE.

SIR,—I am afraid that the tone of the article, Royal Commission and the Indian Medical Service, which appeared in a recent issue of the JOURNAL, is more optimistic than the circumstances should allow. I think the most probable outcome of the inquiry will be, as far as the Indian Medical Service is concerned, simply the holding of the competitive examination for entrance to the Service in India as well as in England, and so the gradual formation of a separate Civil Medical Department recruited in India. Would not this be the very best thing which could happen? We should then cease to see, as at present, men attracted to join the Indian Medical Service under absolutely false conceptions of the conditions of civil employment. We could then have confidence that reasonable terms would be offered in order to attract a sufficient number of candidates to fill the posts on the military side of the Indian Medical Service proper. There are at least two such conditions which must, sooner or later, be considered: (1) Living wage; (2) prospects of the more senior men.

The present pay is not a living wage for a married officer in view of the enormously enhanced prices of the necessaries of life, and this fact was recognized by the Indian Government when it increased the pay of all

regimental officers, other than the medical officer, some four years ago.

To turn to the subject of prospects. What are they? Absolutely *nil*. A man joins, becomes medical officer of a regiment, and as such he must remain; there is no possible chance for him. He will do the same work and have exactly the same responsibility after twenty years' service as he had when an untrained officer of, say, one year or less. As long as the present stupid and effete regimental hospital system continues, this must be so.—I am, etc.,

September 11th. MAJOR, I.M.S.

THE FIRST "INSPECTOR OF HEALTH."

SIR,—I think I may venture to say that the first person who ever bore the definite official title of "Inspector of Health" was Sir Jerome Fitzpatrick, who in 1795 was appointed under this title, to advise the War Office as to the health of the soldiers. This officer was himself a medical officer of the army, but he was not under the Medical Board that in those days administered army medical affairs. Indeed, he was quite independent of the board, and from time to time friction occurred between the board and the inspector in his efforts at preventing disease.

He seems to have effected several reforms for the benefit of the soldier, then so badly cared for. I knew nothing of his existence until I found an excellent mezzotint of him in a print shop in Soho. Very little seems to be known as to who he was or what training in medicine he received. Some brief reports of his work are preserved in the War Office section of the Public Record Office in London.

Perhaps some of your readers could give some particulars of him. He was alive certainly up to 1807. He is sometimes called Sir *Jeremiah* Fitzpatrick.—I am, etc.,

Junior United Service Club, London, Sept. 2nd. GEORGE J. H. EVATT, Surgeon-General (ret.).

ANAPHYLAXIS.

SIR,—I published a paper in the *New Zealand Medical Journal* some years ago on the administration of anti-streptococcus serum per rectum, quoting a number of cases. In one of these cases the treatment was continued for some weeks—I cannot now say how long, as I have not my notes by me. From our present knowledge, we should suppose that, had the serum been given subcutaneously, anaphylaxis would have occurred. This raises the important question, Does rectal administration prevent anaphylaxis? This can only be answered by experiment, and I trust that some one will undertake this important work.—I am, etc.

Bournemouth, Sept. 10th. ALBERT I. GARLAND.

SEA-WATER INJECTIONS IN SUMMER DIARRHOEA.

SIR,—All those who are interested in the use of isotonized sea-water injections in disease will be grateful to Dr. Parry for his letter in the JOURNAL for August 31st.

As one of those responsible for the issue of the statistical report of the Quinton Polyclinic, I particularly wish to thank him for the fair and open-minded manner in which he examines the details of the cases of gastro-enteritis which are therein mentioned. He should, however, remember that a great number of these cases there reported have no details as to the number of days that the patient had been suffering, and that most probably a fair proportion had lasted under seven days.

When you consider the great pressure under which we were working—frequently nearly 200 cases to be attended to in an afternoon—we may perhaps be pardoned that in the heavy stress of work we often omitted to note in our casebooks the number of days that the illness had been running.

It should also be borne in mind that this special method was hardly sufficiently known in this country for many cases to be brought promptly to the dispensary, but the reports of the *Dispensaires Marins* in Paris, where the treatment is well recognized, show a record of about 90 per cent. of cures in infantile cholera.

I would also remind Dr. Parry that isotonized sea-water is used successfully in many diseases besides summer diarrhoea, as any medical practitioner can easily ascertain should he care to attend the work carried on at Poland Street.—I am, etc.,

ARTHUR SANDBERG, M.D.,
Honorary Physician to the Quinton Polyclinic.
London, S.W., Aug. 31st.

NIGHT NOISES IN LONDON.

SIR,—I venture to appeal through your columns to the profession to assist me in moving the authorities to take some steps towards mitigating the intolerable night noises of London and suburbs. If it could be shown that, in addition to their nuisance, they were having, or were likely to have in time, especially if they go on increasing, as they must unless checked, a deleterious effect on the health of a considerable portion of the community, Government might be prevailed upon to appoint a committee to consider the matter. I shall be very happy to have a postcard from any member of the profession who approves my action, and I trust you may be pleased to lend the movement your support.—I am, etc.,

CHAS. MONK, Lieut.-Col. I.M.S. (Ret.).
Barnes, Sept. 11th.

FORCIBLE FEEDING OF SUFFRAGE PRISONERS.

SIR,—It seems to me that the only point in connexion with forcible feeding is to determine whether the proceeding is ethically defensible or not. As a means of saving life in the case of a lunatic who refuses food, no one will deny that it is a right and proper method of treatment. When a doctor is asked to carry out this treatment on a prisoner the question is not one of saving life, but of making it possible to carry out a sentence of imprisonment. In the eyes of the prisoner and the public he is putting himself in the same position as a prison warder. If forcible feeding is latent in every sentence of imprisonment, then the doctor who carries it out is in the same position as the man who carries out any other sentence the law orders—it is simply part of the punishment. The profession has a right to consider whether this is consistent with its dignity. To discuss alternative methods would involve a thorough examination of the whole prison administration, and opens up very serious questions which will have to be settled some day.—I am, etc.,

JAMES W. MCINTOSH, M.B., F.R.C.S.E.
Manchester, Sept. 16th.

DOCTORS AND DISPENSING.

SIR,—Will any member of the Association who has given up dispensing in country practice give me the benefit of his experience as to what he did with regard to fees, and whether his patients seemed satisfied or the reverse?

I have a small country town practice; there is a chemist in the town who would dispense if I were to prescribe. My *lowest* visiting fees are 2s. 6d. in the town, 5s. outside up to 4 miles, and 1s. a mile beyond; medicine always extra, 2s. 6d. a bottle of twelve doses. These are the charges initiated by the founder of the practice and carried down to me. It is a curious way of charging, and I am wondering how I should charge if I gave prescriptions instead of medicine. I feel sure that the only way to check "prescribing chemists" is to stop "dispensing doctors."

This and the more time given us for reading, in these days when so much is to be learnt, should certainly be for the good of our patients, and if we could still keep the wolf from the nursery door it would surely be desirable.—I am, etc.,

WESTCOUNTRYMAN.

PROFESSOR DR. NIETNER of Berlin, general secretary of the German Central Committee for the Prevention of Consumption, will deliver the inaugural lecture at the Medical School for Diseases of the Chest, Royal Hospital for Diseases of the Chest, City Road, E.C., on Thursday, October 17th, at 5 p.m. The subject of the lecture will be The Modern Combat against Pulmonary Tuberculosis amongst Children. A donation of £50 has been received by the Dean from Dr. Horace Dobell towards the Research and Equipment Fund.

Obituary.

REGINALD EDWARD THOMPSON, M.D., F.R.C.P.LOND.,

LATE CONSULTING PHYSICIAN TO THE BROMPTON HOSPITAL FOR CONSUMPTION.

DR. REGINALD E. THOMPSON, who suffered an attack of cerebral haemorrhage in November last, died at his residence at Chelsea on September 10th. He was the son of Mr. Serjeant Thompson, and was born in London in 1834. He received his early education at Brighton College, from which he passed to Trinity College, Cambridge, where he graduated B.A.; he took the degree of M.B. Cambridge in 1860 and graduated M.D. in 1863. In 1868 he became a Fellow of the Royal College of Physicians.

He pursued his medical studies at St. George's Hospital, and soon after obtaining his first degree accompanied the late Viscount Milton on a shooting expedition to the Canadian North-West, then almost completely unexplored. A few years later Viscount Milton made the memorable expedition across the Rocky Mountains, in which he was accompanied by the late Dr. W. B. Cheadle.

Dr. Thompson was appointed Medical Registrar to St. George's Hospital, and during his tenure of this office an outbreak of typhus fever occurred among the tenements which then existed in the neighbourhood of Sloane Street. During this outbreak 120 cases were admitted into St. George's Hospital, and as Medical Registrar Dr. Thompson had an opportunity of seeing them, and the results of his observations were embodied in a paper contributed to the first volume of the *St. George's Hospital Reports* in 1866, in which among other matters he discussed the differential diagnosis of typhus and typhoid fever. He himself contracted the disease towards the end of the outbreak, but made a perfect recovery. In 1869 he was appointed assistant physician to the Brompton Hospital for Consumption, with which institution he continued to be connected for nearly the whole of the remainder of his active life. In 1880 he became physician to the hospital, and for many years the *post-mortem* examinations were made under his supervision. On his retirement in 1894 he was appointed consulting physician, a post which he retained until 1901. In 1871 he was appointed physician to the Seamen's Hospital, but retired two years later. In 1880 he was secretary of the Royal Medical and Chirurgical Society, and in 1883 was appointed vice-president. His chief contributions to literature was a work on *Family Phthisis*, to which, as well as to his contributions on the subject of pulmonary haemorrhages, Dr. Theodore Williams refers below.

Both before and after his retirement he was much occupied in life insurance work, and for many years was a familiar figure at the London medical societies. He was a handsome man and always appeared to enjoy robust health, while his geniality endeared him to a large circle of friends. He was a good musician and a distinguished amateur artist. In recent years he had lived a very retired life, and to many of the younger generation he was personally unknown. He married in 1874 a daughter of Professor Augustus De Morgan, the mathematician, who died many years ago. Dr. Thompson is survived by two sons.

We are indebted to Dr. THEODORE WILLIAMS for the following appreciation of his late colleague:

It was with much regret that I noted the death at the age of 78 of Dr. Reginald E. Thompson, an old colleague of mine on the staff of the Brompton Hospital, and formerly physician to the Seamen's Hospital, Greenwich. It is seldom that I have known a physician of so many accomplishments and a man whose work and whose friendship I have so highly valued. Dr. Thompson was an excellent pathologist, a careful clinician, and a most able reporter. He contributed more than one paper on the results of pulmonary haemorrhage on the lung tissue, a subject which he investigated and illustrated with much success. Another good piece of work was his book on *Family Phthisis*, a statistical inquiry into the cases of the Brompton Hospital from the standpoint of family predisposition, and this has been helpful in the recent study of eugenics and allied sciences. Dr. Thompson was well known as an artist, and he was so good-natured that his talents as a draughtsman were always at the disposal of his colleagues and of the hospital which he served with such whole-hearted fidelity.

DAVID GEORGE DAVIDSON, M.B., C.M.,

EDINBURGH.

WE regret to announce the death of Dr. David G. Davidson, of 9, Granville Terrace, Edinburgh, which occurred quite suddenly at Kinloch Rannoch, in Perthshire, on September 9th, while he was on a short walking trip with an old friend.

Dr. Davidson was born at Lochans, near Stranraer, in Wigtownshire, on May 8th, 1865, the third son of a well-known educationist whose six sons all passed through the University of Edinburgh. After a course of considerable distinction he graduated M.B. and C.M. at Edinburgh in 1889. He gained his wonderfully exact knowledge of anatomy at Minto House under Professor Symington, and he worked under his chief as demonstrator for several years. Later he acted as class and clinical assistant to the late Dr. Alexander Bruce, and he was accustomed to say he had gained all his knowledge of medicine from that great neurologist. After a short period of service under Professor Griffith, of Leeds, he settled down to general practice in the south-west part of Edinburgh; here he laboured for more than twenty years, up to, indeed, the last week of his life. A man of unusual energy and vigour alike of mind and body, he built up a very large practice, but he paid for it by overtaxing his strength, so that he broke down at the early age of 47. He married in 1902, but lost his wife only in December last; he leaves behind him a son of 9.

Dr. Davidson was a very social and lovable man, and he had many devoted friends. He had a very sunny temperament, and carried about him an atmosphere of geniality and happiness that brought hope to the bedsides of his patients. No man, even of his profession, ever did more for nothing, and he had his reward in the love and gratitude of the poor and needy throughout the Fountainbridge district of Edinburgh. Never was a more impressive scene than his funeral. He was carried to his grave amidst the hushed silence and the tears of thousands of spectators who lined the street betwixt his house and the North Morchiston Cemetery. He was buried with military honours, as he had been for some years an active officer in the Edinburgh Territorials. Indeed, Nature had made him for a soldier, and he never looked liker himself than in the saddle, or in his characteristic short and sharp manner of lecturing on ambulance work to railway men or soldiers. He did the full work of two men all through his short lifetime; and perhaps it is better to fall on the field than to linger long as a supernumerary after the battle of life.

Dr. JOHN THOMSON writes:

In Dr. David G. Davidson the profession in Edinburgh has lost one of its most justly popular members. His vigorous and handsome presence and his kindly greeting will long be missed in the district where for more than twenty years he has been so well known and so highly esteemed. His practice was a large and very exacting one, and not a little of his best energy was given to people who never paid him except in gratitude. To his medical brethren he was a thoroughly good neighbour, and his ability, his enthusiasm in his work, his straightforwardness, and his unselfishness made him a man of weight in all gatherings of his own profession. Those who knew the terrible strain through which he recently passed during his wife's long and distressing illness could not but be anxious as to its effects on his health. But his sudden illness was not anticipated, and his death has come as a great shock to his many friends.

Dr. D. J. GRAHAM, R.A.M.C.(T.), writes:

Dr. Davidson was a keen and enthusiastic supporter of the Territorial Force. He was a Captain in the 3rd Lowland Field Ambulance, R.A.M.C., and took a great interest in the welfare of the corps, doing everything in his power to further its interests and maintain its honour. It was at camp that we saw Davidson at his best. He simply

loved being at camp, and at Lanark in July of this year little did we dream that it would be his last camp. There he was full of life and energy, and apparently in the best of health. No one was more active than he was, no one was cheerier. His high spirits and vivacity were infectious, and he was the very life of the officers' mess. By his cheerful, smiling countenance and his inexhaustible store of anecdotes he kept us all entertained and amused. There were no dull moments when Davidson was around. He was beloved by officers and men alike, and little wonder! A more kind, obliging, considerate, generous, and unselfish man never breathed. His large heart and lovable personality endeared him to us all.

We deeply mourn his loss and feel that a bright light has passed from our midst. Long will his memory be kept green in the 3rd Lowland Field Ambulance.

CHARLES EDWARD COVEY, M.R.C.S., L.S.A., ALRESFORD.

CHARLES EDWARD COVEY, who died at Alresford on August 31st, at the age of 71, came of a family which has for many years been well represented in the medical profession. His father, Edward Covey, had an extensive practice in Basingstoke, and was a large landowner in the neighbourhood and a J.P. for Hants.

C. E. Covey was educated in Winchester and at St. Bartholomew's Hospital, where he took the M.R.C.S. and L.S.A., and was house-surgeon to the late Sir James Paget.

After practising for a short time, first at Basingstoke and then at Walton, in Suffolk, he removed to Alresford, taking over the practice of his uncle. There for some twenty-five years, until his retirement in 1904, he carried on a large practice. When he retired he built himself a house in the neighbourhood, in which he lived until his death.

He was a sound and careful surgeon, and a man of most untiring industry. His distinguishing characteristics were an unusually benevolent and sympathetic nature, which caused him to be universally beloved, especially by his poorer patients, and a straightforward honesty which earned the respect and confidence of every one who had to do with him.

His leisure moments—few enough in the strenuous years of his middle life—were devoted to sport, for which he had a passion which survived undimmed until the day of his death. There is scarcely any branch of outdoor sport in which he had not taken part, but it is as a cricketer and a shot that he will be best remembered.

His cricketing days were over before he met the present writer, but the latter can speak from experience of his capabilities as a shooting man. Defective sight prevented his being a very brilliant shot, but in his knowledge of the habits of the game he hunted and of the best way to bring them to bag he was quite unsurpassed.

He belonged to a vanishing type of sportsmen who owed the size of their bags to their knowledge of the art of venery rather than to their power of drawing cheques; and the familiar figure of the old doctor trudging home with his gun on his shoulder and his dog at his heel will long be remembered by his neighbours with affectionate regret.

WE regret to have to record the death on August 16th of Dr. WILLIAM FLOOD, of York. An Irishman by birth, he was born in 1860, and received his medical education at the Ledwich School, Dublin, and at Queen's College, Galway, obtaining in 1882 the Licence in Medicine, Surgery, and Midwifery of the Royal Colleges of Edinburgh. He had been for some twenty years resident in York, where he had a large practice, especially among its poorer inhabitants, and was well known for his kindness towards them. Early in the present year laryngeal trouble arose subsequent to a severe cold, and though he went away for change of air on several occasions he never entirely recovered, and for some time previous to his death had ceased to carry on his practice personally. Dr. Flood was married and is survived by his wife and three daughters.

FLEET SURGEON ALFRED H. LISSANT COX, R.N. (retired), died on August 28th. He studied at King's College, and took the diplomas of L.S.A. in 1885 and M.R.C.S.Eng. and L.R.C.P.Lond. in 1887. He joined the service as Surgeon in August, 1889, and became Staff Surgeon in August, 1897, serving in that rank in the cruiser *Fox* during the trouble at Bebrein and Hodeida in 1905; he was promoted to the rank of Fleet Surgeon in August, 1905. From October, 1909, to October, 1911, he served in the *Achilles* and then joined the *Blake*. He retired in February, 1912.

DEPUTY INSPECTOR-GENERAL GEORGE KELL, R.N. (ret.), who died at Southsea on September 9th, took the diplomas of L.R.C.P. and L.R.C.S.Irel. in 1863. He entered the service in March, 1867, being appointed Staff Surgeon in September, 1877, and Fleet Surgeon in June, 1886. He retired in November, 1895, with the honorary rank of Deputy Inspector-General.

SURGEON-MAJOR BERNARD KENDALL, Bengal Medical Service (retired), died suddenly at Upper Norwood on September 3rd. He was born on June 21st, 1831, took the M.R.C.S. in 1853, and entered the Indian Medical Service as Assistant Surgeon on August 4th, 1855. He became Surgeon on August 4th, 1867, Surgeon-Major on July 1st, 1873, and retired on February 22nd, 1879. He served in the Indian Mutiny in 1857-8. The Indian Army List shows no fewer than fifty-two officers of the Indian Medical Service as still surviving, on the retired list, who were in India in or before January, 1858, and so were in the country at the time of the Mutiny. Only twenty-five of them, however, appear to hold the Mutiny medal for actual war service in the campaign. Two of them—W. H. Harris and B. Williamson—also served in the Crimea. Only one—W. F. Mactier—served in the Sikh war of 1845 and the Punjab campaign of 1848–9, as well as in the Mutiny. The senior officer shown as still alive on the retired list—Surgeon-Major Henry Benjamin Hinton—entered the service on January 14th, 1839, so was actually in India at the time of the first Afghan war, though he did not himself take part in that campaign. He served in the Gwalior war of 1843–4; in the Sikh war of 1845–6, being present at Badiwal, Aliwal, and Sobraon; in the Punjab campaign of 1848–9; and in the China war of 1858–60; but does not appear to hold the Mutiny medal. He was born on March 7th, 1813, so, if he is still alive, must be within six months of the century. Curiously, another retired Bengal medical officer of the same name—Thomas Lambert Hinton—completed his century a few years ago; he was born on May 1st, 1808, and died at St. Leonards on June 14th, 1908.

DEATHS IN THE PROFESSION ABROAD.—Among the members of the medical profession in foreign countries who have recently died are Professor Ludwig Back, the well-known ophthalmologist of Marburg, aged 46; Dr. Josef Disse, Professor of Anatomy in the University of Marburg, and formerly Professor in the Imperial University of Tokyo, aged 59; Dr. Maurice Howe Richardson, Professor of Surgery in the Harvard Medical School, and Surgeon-in-Chief to the Massachusetts General Hospital, and author of many contributions to surgical literature, in his 61st year; Professor Oskar Eversbusch, Director of the Clinic for Diseases of the Eye in the University of Munich, aged 59; Dr. W. Bender, of Camburg, a well-known German bacteriologist, aged 65; Dr. J. E. Newcomb, Professor of Laryngology at the Cornell University, consulting laryngologist to the Roosevelt Hospital and author of various contributions to the literature of his speciality, aged 55; Professor Aurel von Török, of Budapest, the distinguished anthropologist, in his 70th year; Professor Cramer, Director of the Psychiatric Clinic of the University of Göttingen, aged 52; and Dr. C. E. F. Monoyer, Professor of Medical Physics in the Medical Faculty of Lyons, aged 76.

AN association of mental nurses working under the title of the New Mental Nurses' Co-operation has recently established itself at 85, Edgware Road, not far from the Marble Arch. All the members have been carefully chosen with a view to their suitability for work as private nurses, all have had prolonged experience in asylums, and all possess certificates testifying to their ability in their particular branch of nursing work. The superintendent is Miss M. E. Cook, who will doubtless be ready to afford any information desired by inquirers.

Medico-Ethical.

The advice given in this column for the assistance of members is based on medico-ethical principles generally recognized by the profession, but must not be taken as representing direct findings of the Central Ethical Committee, except when so stated.

CHANGE OF ADDRESS.

G. B. M.—We should advise our correspondent to send a circular headed "Change of Address," which may be printed on a postcard, to all bonâ fide patients of his practice and to all medical friends whom he wishes to notify.

PATIENTS AND PRACTITIONERS.

NESCIUS.—(1) It would have been better if B. had said nothing to X. as to A.'s conversation with him, as X. not unnaturally resented being made to feel that he had somehow parted with his inherent right to consult any medical men he chose, and that he was regarded as having become the property of A. (2) After what had passed B. was inconsistent in attending the third case, but the right of X. to consult him should not be disputed, as the public will never submit to the proposition that having once called in a certain practitioner to attend one member of a family they are for ever precluded from seeking the advice of any other member of the local medical profession.

FEES TO MEMBERS OF THE MEDICAL PROFESSION.

D. P. M.—We have always found medical men of all ranks willing to give their time and services gratis to brother practitioners if asked to do so, but we think any arrangement for reduced fees should be made before and not after these have been incurred, especially when, as in this case, the patient is in relatively good circumstances and is not dependent on practice. The only unsettled question seems to be the pathologist's fee for making the vaccine; we do not think three guineas excessive considering the trouble of getting a satisfactory result.

The Services.

ROYAL NAVY MEDICAL CORPS.

DEPUTY INSPECTOR-GENERAL S. SWEETNAM has been awarded the Greenwich Hospital pension of £50 a year, rendered vacant by the death of Deputy Inspector-General J. S. Dobbyn.

INDIAN MEDICAL SERVICE.

SUB-ASSISTANT SURGEONS.

WITH effect from May 13th, 1912, the Government of India have sanctioned the following enhanced rates of pay for military sub-assistant surgeons:

Senior Sub-Assistant Surgeons.	
First class, ranking as Subadar ...	...Rs.110
Second class, ranking as Jemadar	... 90
Indian Warrant Officers.	
Sub-Assistant Surgeons, first class	... Rs.70
Sub-Assistant Surgeons, second class	... 50
Sub-Assistant Surgeons, third class	... 35

The above rates are inclusive of the extra pay for an English qualification.

Public Health

PAIL CLOSETS AND TYPHOID FEVER IN NOTTINGHAM.

THE direct connexion between pail closets and the incidence of typhoid fever has been demonstrated by many observers. In successive annual reports Dr. P. Boobbyer, the medical officer of health of the city of Nottingham, has shown that both this disease and diarrhoea occur far more frequently in houses and districts where such closets are in use than in those furnished with waterclosets. In a special report recently issued he points out that the liability of pail closets to foster and disseminate infection is explained by the fact that the specific virus of typhoid fever, epidemic diarrhoea, and other diseases, is capable of surviving for considerable periods in faecal matter and in soil, and that these closets, even with the most careful management, necessarily entail the storage of faecal matter in the immediate vicinity of dwellings and the pollution of the latter and their surroundings by such matter, to a very large extent in many cases and to some extent in all. While admitting that there are innumerable other agencies by which typhoid fever is fostered and propagated in large centres of population, and notably by means of human carrier cases, he maintains that the pail closet is a powerful factor in this maintenance and propagation, and as such should be eliminated as speedily as possible. Incidentally he points out that a continuance of this conservancy system might involve a sanitary authority in heavy financial responsibility under certain provisions of the National Insurance Act. It appears that there are in Nottingham over 36,000 pail closets, and the cost to the authority of scavenging them amounts to about £24,000 per annum. Quite apart, therefore, from the sanitary improvement which would be effected by their abolition it would seem that in a very few years there would be a substantial financial saving. The Nottingham Corporation will be well advised to give speedy effect to the advice of their medical officer of health.

Medical News.

ON the occasion of the opening of the winter session at St. Mary's Hospital Medical School by the Lord Mayor of London, his lordship will visit the hospital to inspect the casualty department, where structural improvements have just been completed, and will declare the department open.

THE medical men who have attended the demonstrations at the Kennington Road Tuberculin Dispensary are invited to take part in offering a complimentary dinner to Dr. Camac Wilkinson on October 7th. Further information can be obtained from Dr. A. White Robertson, 26, Harley Street, W., who asks that notifications to attend may be received by September 30th.

BY way of commemorating the opening of its new buildings, the Royal Insurance Company has published a pamphlet containing an account of the history of Lombard Street, in which it established itself in the first half of last century. Lombard Street appears to have been recognized as a centre of financial operations even earlier than the thirteenth century, and has retained its repute to the present date despite all structural and other changes in the city. The pamphlet is illustrated by reproductions of maps, prints, and engravings preserved in the Guildhall and elsewhere.

As has already been announced in the JOURNAL, the first International Congress of Comparative Pathology will be held at Paris in October (17th–23rd), under the presidency of Dr. Roger, professor of experimental and comparative pathology in the Paris Faculty of Medicine. This congress will deal with the whole series of diseases common to mankind and animals, with the relations existing between the diseases of the different species, with vegetable pathology and the possible relations of certain diseases of plants and those of animals. In addition to reports on the pathogeny of tuberculosis to be submitted by MM. Calmette, Chaussée, Vallée, and Lignières, other communications on the same subject will be presented by Professor Paul Courmont of Lyons (comparison of agglutination of Koch's bacillus in man and animals); A. De Jong, professor of medicine in the University of Leyden; Dr. Moore, director of the Veterinary College, New York (elimination of tubercle bacilli by infected animals); Professor Bruschettini of Genoa (vaccination against bovine tuberculosis, studied on laboratory animals—rabbit, guinea-pig, etc.); Dr. André Jousset (tuberculosis in the guinea-pig), and others.

THE issue of the *Gesundheitslehrer* for August contains the following story: A lady who had journeyed from her home in Switzerland to Riga had the misfortune to fall ill there of an affection which was diagnosed as chicken-pox. For this she was kept in a hospital in Riga. She went on to Frankfurt, where two persons who had previously been vaccinated fell ill with modified small-pox; a child of one of them had recently been vaccinated and escaped infection. The doctor who attended these two persons, a Dr. Spohr, was a well-known antivaccinator. He had never been vaccinated, and suffered a very severe attack of variola, for which he was nursed by his wife at home. Mrs. Spohr, however, had been vaccinated, as had the Spohr children. According to one account, the vaccination of the children had been carried out by their father, and in one case the success was doubtful. Be this as it may, the only other member of the Spohr family infected was the child whose vaccination was described as doubtful. In the meantime, Dr. Spohr considered it advisable for his health to convalesce in Switzerland. The child's attack was mild. Three further cases occurred in the neighbourhood, but by this time the authorities got wind of the state of affairs and promptly stepped in. The patients were removed to the isolation hospital, and as many persons as were suspected of having been in contact with the infected were forthwith vaccinated. But it was too late to prevent some further spread, and, in all, 15 cases have been traced; of these, one proved fatal, while another was reported to be dangerously ill. It will be interesting to learn whether Dr. Spohr returns to Frankfurt after his completed recovery, and, if he does, what the authorities will have to say to him.

Letters, Notes, and Answers.

AUTHORS desiring reprints of their articles published in the BRITISH MEDICAL JOURNAL are requested to communicate with the Office, 429, Strand, W.C., on receipt of proof.

TELEGRAPHIC ADDRESS.—The telegraphic address of the EDITOR of the BRITISH MEDICAL JOURNAL is *Aitiology, Westrand, London.* The telegraphic address of the BRITISH MEDICAL JOURNAL is *Articulate, Westrand, London.*

TELEPHONE (National):—
2631, Gerrard, EDITOR, BRITISH MEDICAL JOURNAL.
2630, Gerrard, BRITISH MEDICAL ASSOCIATION.
2634, Gerrard, MEDICAL SECRETARY.

☞ *Queries, answers, and communications relating to subjects to which special departments of the* BRITISH MEDICAL JOURNAL *are devoted will be found under their respective headings.*

QUERIES.

DR. REID (Aberdeen) desires to hear of an institution on the Continent for the treatment of a moral case in a lad 18 years old.

CORNISHMAN desires to hear of an efficient, simple, and cheap appliance for a case of repeated dislocation of the shoulder in an epileptic, and of any charitable society which would supply the same in a deserving case, either gratis or at a reduced cost.

NORTHERN finds that water from a certain well, even when boiled, is apt to cause gastric disturbance and diarrhoea. Long fibrous roots, probably of a lime tree growing near, protrude into the well, and he wishes to know whether they are likely to render the water injurious.

*** Whatever the answer to this question, the sides of the well should be cemented, or made watertight in some other way, down to the water level.

INCOME TAX.

G. F. C. W. has a salary of £218 per annum as assistant, £191 being paid in cash and the remainder deducted for furnished quarters. He wishes to know what sum to return for income tax.

*** The law is that only money and what can be turned into money, is chargeable to income tax. If it is part of our correspondent's agreement with his principal that he shall receive £191 in cash and the remainder of his nominal salary in the form of the benefit of furnished quarters, then only the cash received is returnable for income-tax purposes. If, however, the agreement is for a salary of £218, and the arrangement as to furnished quarters is an optional arrangement quite outside the agreement, and terminable at will, the deduction from the salary is merely a convenient method of settling a debt, and the full salary should be returned.

ANSWERS.

H. B.—An excellent general discussion of the nature and treatment of pulmonary tuberculosis will be found in Dr. Hyslop Thomson's book on *Consumption in General Practice* (reviewed September 14th). All that it is essential for nurses to know about it will be found in *Practical Nursing*, by Miss Isla Stewart and Dr. H. E. Cuff.

ALIENIST.—(1) Psycho-analysis is described in the following publications: *Selected Papers on Hysteria and other Psychoneuroses*, by Professor S. Freud. *Three Contributions to Sexual Theory*, by Professor S. Freud, with an introduction by Professor I. J. Putnam. All these have been translated by Dr. A. A. Brill, and are published by the Journal of Nervous and Mental Disease Publishing Co., New York. Mr. H. K. Lewis is the London agent. Dr. Brill has now in the press a translation of Freud's *Die Traumdeutung*. Dr. Bernard Hart has published "Psychology of Freud and his School" (*Journal of Mental Science*, April, 1910), and two articles on the educative aspects, by Dr. E. Jones and the Rev. Dr. Pfister respectively have been published in *School Hygiene*, February, March, July and August, 1911. (2) Among the most practical books on hypnotism are: *Hypnotism and Treatment by Suggestion*, by J. Milne Bramwell (Cassell and Company); and *An Introduction to the Study of Hypnotism*, by H. E. Wingfield (Baillière, Tindall and Cox).

ASYLUMS AND PAYING PATIENTS.

C. B. F.—Of public asylums the following, in or near London, take patients within the terms named: (1) Bethlem Royal Hospital, Lambeth Road, S.E.; (2) St. Luke's Hospital, Old Street, E.C.; (3) City of London Mental Hospital, Dartford, Kent; (4) Croydon Mental Hospital, Warlingham, Surrey. Of private asylums: (1) Bethnal House, Cambridge Road, N.E.; (2) Camberwell House, Peckham Road, S.E.; (3) Peckham House, 112, Peckham Road, S.E. Near London is the Holloway Sanatorium, Virginia Water, Surrey. Particulars as to any of these can be obtained from the respective secretaries or medical superintendents.

TEETH GRINDING.

DR. J. E. MIDDLEMISS (Bradford) writes: In the absence of any demonstrable organic lesion or reflex cause, the condition described by "Perplexed" in your last issue would appear to be a neurosis akin to somnambulism, and I would suggest that it be treated by hypnosis. This method was adopted by me recently in a case essentially similar to the one propounded—namely, that of a girl otherwise in excellent health, but who had a persistent habit of talking loudly and constantly in her sleep in such a manner as to disturb the whole household, and to constitute a real nuisance. This was treated by hypnotism, and after some seven or eight sittings the condition almost entirely disappeared. In this case, too, the patient was quite unaware of her rather distressing habit. If hypnotism is objected to, the method of suggestion during the waking state might be employed, but in a habit of long standing is hardly likely to be so successful as the more powerful method.

LETTERS, NOTES, ETC.

WHEN opening the discussion at the Liverpool meeting on the education of the specialist in laryngology and otology, Professor Holger Mygind of Copenhagen classified Continental universities according to the extent of the teaching in oto-laryngology which they at present officially supply. He now writes to say that the figure relating to the total number of universities in Holland should have been 4, not 5, and the number in Belgium 5, not 4. The paper will be found at p. 413 of our issue for August 24th.

PED. writes that he can confirm the view, once more commonly held than it is to-day, that the addition of a little cream to the milk given to marasmic infants suffering from diarrhoea and vomiting, will sometimes be followed by remarkable improvement.

MASONIC CHARITIES.

DR. WILLIAM WILSON (Secretary, St. Luke's Medical Lodge of Instruction, 184, Goldhawk Road, W.), writes: May I again, through the medium of your widely-read columns, appeal to medical men, and medical Freemasons in particular, for votes for the Masonic charities? At the forthcoming elections there are several medical candidates. Votes for any of the Masonic institutions are equally acceptable, as exchanges can be effected, and will be gratefully received and acknowledged by me.

SIR CHARLES TUPPER, BART.

DR. CLIPPINGDALE writes: I find Sir Charles Tupper is both M.D. and L.R.C.S.Edin. The venerable baronet, however, appears to have left these qualifications unregistered; hence the absence of his name from medical directories and from the list of medical baronets published in the JOURNAL of May 25th.

A HOLIDAY EXPERIENCE.

DR. R. WARING TAYLOR (Gomersal, near Leeds) writes: Whilst on my holiday this year I came across an American surgeon in a difficulty. He had come over with a party of friends, and a lady of the party had got an attack of appendicitis. He decided to operate, and after a long interview with the American Consul in Antwerp, it was found only to be possible for him to do so on the condition that a Belgian surgeon on a hospital staff would consent to be present and take the responsibility. The operation was done in a nursing home, and nothing could have been nicer. The nurses were nuns. The curious point to me was that the Belgian law should insist on a registered practitioner being present as cover to three men who were not registered. If it had been done here it might have cost our friend his position on our own *Register*. The American was helped by his brother, who gave the anaesthetic, and by me, who acted as house surgeon. The Belgian looked on and approved. The surgeon said that he was proud to have made a record in the annals of surgery in Antwerp, as he was sure that it was the first time an Anglo-Saxon team had worked in threefold harness in Belgium.

THE INSURANCE ACT.

He who's insured against his will
Will find it pays him to be ill,
For no "insurance tout" can tell
What gain he has by keeping well.

A. D.

SCALE OF CHARGES FOR ADVERTISEMENTS IN THE BRITISH MEDICAL JOURNAL.

	£	s.	d.
Eight lines and under	0	4	0
Each additional line	0	0	6
A whole column	2	13	4
A page	8	0	0

An average line contains six words.

All remittances by Post Office Orders must be made payable to the British Medical Association at the General Post Office, London. No responsibility will be accepted for any such remittance not so safeguarded.

Advertisements should be delivered, addressed to the Manager, 429, Strand, London, not later than the first post on Wednesday morning preceding publication, and, if not paid for at the time, should be accompanied by a reference.

NOTE.—It is against the rules of the Post Office to receive *poste restante* letters addressed either in initials or numbers.